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Editorial.

HEREDITY.

I DESIRE to draw my readers' attention to two papers on this subject which appear in the present number of THE JOURNAL. Professor Thomson treats the subject more extensively than Professor Hamilton, but the latter restores the balance by treating it more intensively. It is advisable to read Professor Hamilton's minutely reasoned article first, if one is yet imperfectly acquainted with the subject, in order to understand in detail and in particular manifestations the difficulties which beset the investigator, and then, having realised these troubles and dangers, to proceed to the other paper, which has a wider horizon and loftier survey, and enables the reader to summarise his impressions and arrive at a definite conclusion—of hope or despair. In any case, it is good to see the limits, even the apparent limits, of one's possibilities in the region of science. It is a good thing to examine carefully and minutely the trees of a forest, and to learn to distinguish the one species from the other in form and structure; it is also a good thing, and it conveys a higher and different kind of pleasure, to climb a lofty hill, and from its serene summit contemplate the forest that lies beneath us, and mark its limitations.

It will be noted that both writers are, like wise men, careful to define the term "heredity." Nothing is so productive of disputes, so wasteful of labour, and so generally distressing, as looseness of language. How often do we find the same words used in different senses by different people; how often do terms overlap each other, and how often is the result vanity and vexation of spirit! Professor Thomson makes a protest at the very beginning of his paper, and tells us sturdily that heredity is "no power, but a convenient term for the relation of organic or genetic continuity which binds generation to generation." And Professor Hamilton is careful to distinguish between heredity and uterine contagion, contrasting in this respect the influence of tuberculosis and of syphilis. The distinction is a fine one, and requires careful observation, minute experiment, and great accuracy of thought and expression. Professor Hamilton takes great care also to point out the difference between heredity

properly so-called and the habit of body which predisposes the individual to the disease which afflicted his parents or ancestors. He sees quite clearly the difficulty which surrounds the question; he acknowledges that "thin partitions do their realms divide" when he contrasts this habit of body with heredity. He sails perilously near the wind, however, when he asserts that the habit of body may be fostered by "every external agent which tends to deteriorate the natural powers of resistance," and then, a little further down, talks of such habits as having been produced far back in our history, as having been produced irrespectively of external agencies, and as being intensely hereditary. This theory perhaps suggests more questions than it answers. How, for example, did the variation arise if independent of external agencies? Why does it not occur now in the same spontaneous way? Why does it now yield to the seduction of external agencies? And so on.

Mr. Thomson towards the end of his paper expressly disclaims responsibility as to theorising on the transmissibility of *acquired* characters, that being at present an unestablished fact, and he contrasts this with heredity. But does this mean that we are going to revert to the mythical theory of creation? Were living beings all turned out ready-made, each species with its own list of characteristics, which alone could be transmitted? Who is to decide between original and acquired? Was there a time when, say, the pigeon became definitely the pigeon, when it completed the structure of its character, after which time every variation was superficial, transitory, intransmissible?

I may return to this subject at another time. Meanwhile I commend the two papers of my learned contemporaries as healthy stimulants to all earnest men.

Original Papers.

ON HEREDITY IN DISEASE.[1]

BY D. J. HAMILTON, M.B., F.R.S.ED., PROFESSOR OF PATHOLOGY IN THE UNIVERSITY OF ABERDEEN.

ALLOW me, in the first place, to thank the Council of the Society for the very great honour they have conferred upon me in asking me to take part in this discussion, and more particularly for the place they have assigned to me in the order of debate. When I think of the many members of this Society who have made heredity a special study, I feel the honour to be all the more flattering.

It will be acknowledged on every hand that the subject you have chosen for debate is full of interest; an interest which has become, if anything, more intense in late years than formerly, from the vast amount of literature, biological and purely medical, which has found its way into

[1] Paper read before the Edinburgh Medico-Chirurgical Society on March 7th, 1900.

our midst, dealing not only with concrete examples, or supposed examples, of acquired characters being hereditarily transmitted, but going to the very root of the matter in speculations as to what heredity depends upon, and of what the mechanism consists which underlies its manifestations. It is one of the commonest experiences of life that not only the general conformation of the body may be transmitted from parents to child, but that the same holds good of the minutest details of conformation. Further, it is matter of common note that the most subtle common features and traits of character may, similarly, be handed on from one generation to another. We have little experience of how far the finer manifestations of heredity prevail in unicellular organisms, but in the higher metazoa the phenomena are so blatant as to have attracted attention in all ages.

The subject of discussion this evening is the influence of heredity in disease, and here it will be advisable to recall the fact that mere congenital transmission of disease from either parent to child is not heredity. Most of those diseases which are transmitted to the fœtus *in utero* are of the contagious type, and, consequently, are to be referred back to the action of a microphyte. Such transmission of contagious disease is, of course, to be accounted for by direct inoculation of the fœtus, in most cases through the system of the mother. It used to be supposed that the placenta was a perfect filter against the transmission of contagion of the particulate kind; it was taught, for instance, that anthrax is never conveyed from the blood of the mother to that of the embryo. And although the approach of the placenta to perfection in this respect is undoubted, yet now and again it fails, and the contagion passes over to the offspring. Diseases of the contagious type seem to differ in the facility with which they are transmitted by this means. Thus, in the case of anthrax and tuberculosis, the infection of the fœtus through the mother occurs only very rarely, while we know that in that of syphilis the liability is extreme. These cases, however, of intra-uterine contagion hardly come within the scope of our discussion, unless in so far as they can be utilised for the purpose of proving or refuting the alleged transmission of a disease which has been acquired.

Of all the numerous theories to account for the handing down of hereditary characteristics, there may be said to be two which have met with most acceptance; not that they have much in common, but rather because they represent the two points of view from which the matter is regarded. Indeed, it may be said that nearly all other theories which have gained currency are to be looked upon as modifications or extensions of these. The one, it will be remembered, is known as the "Pangenic Theory" of Darwin, propounded by its author, with his characteristic modesty, as a mere provisional or tentative explanation of how acquired features may be transmitted; the other, that of Weissmann, known as the theory of the "Continuity of the Germ-plasm."

Between what we recognise as living protoplasm and its ultimate molecules, there are probably several stages of subdivision, molecular groupings, to the lowest of which various names have been given in keeping with certain theories of vitality bound up with their supposed existence. Thus these supposititious supra-molecular bodies have been termed pangens (de Vries), gemmules (Darwin), micellæ (Nägeli), idioblasts, (Hertwig), plasomes (Wiesner), and biophores (Weissmann). They all refer to what may be regarded as molecular aggregations, which, by their

association and interaction, go to make up what is roughly termed in biological language "living protoplasm." According to Darwin, these excessively minute particles of living matter, "gemmules," as he named them, are constantly being given off from the body-cells and circulate freely throughout the system. They were reputed by him to collect in the reproductive-cells, and to imprint upon these the features which the somatic cells had acquired through external agencies. Thus they tended to proceed centripetally, to convey to the reproductive-cells those impressions which had been received from without, and to transmit these to the offspring. The inheritance of acquired characters, according to Darwin's hypothesis, was therefore habitual.

Weissmann, on the other hand, takes the view that germ-cells (ova and spermatozoa) and somatic-cells differ, in so far, that the germ-cells are the exclusive bearers of hereditary qualities, the transmission being effected through the instrumentation of what he calls the "germ-plasm." This "germ-plasm" is comprised in the chromosomes of the ovum and spermatozoön, is continuous from one individual to another, and is, therefore, practically immortal. He would regard the germ-cells as an exclusive royal line, as it were, handed on from generation to generation, and having no community, so far as the propagation of hereditary qualities is concerned, with the cells of the soma or perishable part of the organism. All new hereditary features are the result of variation in the structure of this "germ-plasm," and are brought about by the recurrence of slight inequalities of nutrition. He ignores the influence of external agencies acting upon the somatic-cells as an element in the production of heredity; he believes that only those variations which are blastogenetic become hereditary, those which are somatogenetic are not so. His theory supposes that the particulate elements of heredity proceed exclusively in a centrifugal direction, and is therefore opposed to that of Darwin. The inheritance of acquired characters, according to his theory, is impossible.

It will be evident that the whole matter of the alleged transmission of disease hereditarily hinges upon this debated point, as to whether acquired characteristics can be inherited; in other words, whether external agencies can so impress the soma, and through it the "germ-plasm," that the effects of these impressions become hereditary. Hæckel, Virchow, and Eimer are at one with Darwin in the belief that they can, and the evidence they have brought forward in support of the assertion is voluminous, in many cases striking, but by no means in all instances convincing. Virchow is a strong upholder of the theory of *causæ externæ* generating effects which are inherited, and it matters not whether these act on germ-cell or body-cell. "A living being placed under varying circumstances changes its functions and habits, and what it acquires it can transmit hereditarily." On the other hand, we have Weissmann, Ziegler, and others, just as strongly convinced that acquired properties are not inherited, but that hereditary tendencies are due to variation in the structure of the "germ-plasm."

The supposed transmission of *mutilations* I shall pass over in a single word by asserting that there is no evidence in support of the view. The Jewish and other Eastern nations have been circumcising since the days of Abraham, and probably long before that period, and I have yet to learn that Jewish children are born without foreskins.

Can we say, however, that an external agent of any kind giving rise to a morbid state of the body, ever so impresses the cells of the body that

the disease becomes established as a hereditary peculiarity? Let me take up some of the best known examples of so-called hereditary diseases with the view of discussing them, and of calling forth the valuable experience which members of this Society must be able to bring to bear upon the subject.

Perhaps no disease is held to have so hereditary a tendency, at least in the layman's mind, as tuberculosis; and it is undeniable that the disease affects different members of the same family, and that the predisposition to it can be traced back in the ascendants as a distinct family strain. If I had come before this Society previous to the discovery of the tubercle bacillus, and had asserted that the disease is not hereditary and very seldom congenital, my remarks would have met with little credence. Yet such is the general conclusion of pathologists at the present day. With extremely few exceptions—so few that they may almost be neglected—children are not born tubercular even of tubercular mothers, nor are the young of the lower animals born tubercular under like conditions. Even in these exceptional cases there is the possibility that the mother suffered from genital tuberculosis while pregnant, the transmission of the disease under such circumstances to the developing embryo being, one would say, a very likely accident. The fact, however, that such contamination of the embryo within the tissues of the mother happens so seldom, shows with what difficulty the transference is effected. It has been asserted, but proof is certainly wanting in confirmation of the allegation, that the ovum may become infected through the spermatic fluid emanating from a person suffering from tuberculosis of the testicle or its adnexa. That the ovum at the time of impregnation may become inoculated, that in a manner it may become fertilised by the spermatozoid and inoculated with the bacillus of tubercle simultaneously, I cannot imagine to be true. After conception has occurred, and the embryo is fairly advanced in development, there is a possibility of such an occurrence, but to my mind the liklihood of its taking place even then is extremely small.

Putting, therefore, the view of tuberculosis being congenital out of court, we have to fall back upon the explanation of the undoubted proclivity which the disease has to run in certain families as dependent upon one of two factors—either direct inoculation of the individual in extra-uterine life, or the inheritance of a particular predisposition, the tubercular temperament. No doubt when a member of a household becomes tubercular, the tendency to infection of other members, through family relationships, is very great, but this cannot be held to be the only cause of the disease selecting such families, and, it may be, affecting several generations in succession. Every physician and surgeon knows that the members of such families are, in a large number of instances, notable as tubercular subjects from their conformation of body and the liability they manifest to certain diseased conditions, not necessarily tubercular, but which nevertheless are sufficiently striking to confirm the suspicion of the hereditary taint. I need not enter into details of what this habit of body is characterised by; the features I am referring to are matters of daily experience with you all. My argument is that it is this habit of body, not the disease itself, which is inherited; and if you asked me in what the particular vulnerability consists, I should reply that, most likely, it resides in the epithelial protective coverings of the body being too little resistant, too easily stimulated by external agencies, too readily penetrated by the parasite

of the disease. There is good reason to believe that, instead of Man being an animal very prone to tuberculosis, he is extremely insusceptible, otherwise it is difficult to explain how tuberculosis has not utterly decimated the human race. When tuberculosis is freshly introduced among savage communities, such decimation has been known to occur. Civilised nations have probably become in a manner hardened against the ravages of the parasite. There is no reason to believe, however, that any member of the human race is immune to its influence, in the sense that certain animals are immune to parasitical diseases which are peculiar to Man, or in the sense in which an animal may be rendered immune artificially. There is good reason for affirming, on the contrary, that there are few members of the human race who could not be inoculated with the tubercle bacillus were it implanted in their tissues.

The lung is the portal at which the organism usually gains admission, and here, it will be remarked, the protection of the surface consists simply of a delicate film of epithelial cells. In children and young persons with the tubercular habit of body, this epithelium tends constantly to be thrown into a state of germination, rendered apparent by slight attacks of bronchitis and catarrhal pneumonia, the desquamation accompanying these conditions exposing the underlying parts and thus encouraging the bacillus to take hold upon them. We have analogies bearing upon this in the ease with which the phylloxera penetrates the epidermis of the French vine, and in the difficulty it has in making its way through that of the American vine, which is thicker and more resistant. The organisms of putrefaction, and probably of septic disease, may be introduced into the healthy bladder of an animal, or that of Man, almost with impunity; while, if introduced into one whose epithelium is in a state of catarrh, and whose deep parts, consequently, are exposed, the danger of communicating septic disease is extreme. We do not know whether, in the case of individuals with the tubercular habit, the epithelia in other parts of the body, such as that covering the intestine, are to a like degree vulnerable. The fact that so many of us can consume tubercular milk without becoming tubercular, would tend to show that, under normal circumstances, the power of resistance possessed by the alimentary canal against this parasite must be very great.

What I would suggest, therefore, as inherently bound up with the hereditary tendency of tuberculosis, is this vulnerability of the protective epithelia, and this, there is every reason to believe, is handed down from generation to generation. In support of this assertion are to be taken into account certain epithelial manifestations which accompany the tubercular habit—namely, the very dark or very light degree of colour of the hair, the overgrowth of hair in the bushy eyebrows and long eyelashes, and, lastly, the occurrence of a lanugo-like overgrowth in tubercular children along the spine and over the legs. To my mind, these all point to an anomaly of the epithelial type which is peculiar to the tubercular habit of body.

Granted, for the sake of argument, that this habit of body, which is handed on from one generation to another, is the cause of the hereditary disposition to tuberculosis, and that it manifests itself in certain peculiar anomalies of the epithelia, we may ask, in reference to the subject of debate, how it is that this habit of body has arisen? Where has the inherited strain come from? What is its ancestral history? Can it be generated by vicious surroundings? I question whether it can. No

doubt, once in the blood, the particular habit may be fostered by every external agent which tends to deteriorate the natural powers of resistance. But will such external agencies tend to produce a particular colour of hair, a certain narrowness of chest, tallness of stature, and other peculiarities which are distinctive of the tubercular constitution? My conviction is that they will not, and that we must go much further back in the history of the human race to get at the explanation of the matter. My own impression is that these features are the lineal descendants of a variation which took place far back in our history, that the variation has occurred irrespective of surroundings or external agencies, and that its influence has been propagated in the descendants ever since. It may be a variation which is common to many races, but one which apparently is intensely hereditary. When we see how racial peculiarities are propagated for ages, how the type of character no less than the lineaments of a race continue very much the same through all time, is it very Quixotic to suppose that a certain type of constitution has in a like manner become a race inheritance? It seems to me, on the contrary, to take a concrete example, that if the spirit of commercial enterprise exemplified in that memorable transaction between Abraham and the children of Heth concerning the purchase of the cave of Machpelah be still the spirit of the Jewish nation, there is no reason to believe that a variation of structure predisposing to disease may not have been handed down to us from remotest times.

The second so-called hereditary disease which I wish to bring before your notice for critical examination is gout. There is no more common belief than that this disease is engendered of high living, and that, once established, it is capable of being hereditarily transmitted. In a certain sense this notion is correct, and in a certain sense it is, to my way of thinking, entirely erroneous. There is no doubt that once the gouty tendency has been made manifest by the external agencies referred to, the disease will most likely show itself in the next generation, but not necessarily so; it may skip a generation and reappear in that which follows. But are we quite sure that the gouty habit—and I employ this term in its widest sense—has originated in the abuse of articles of diet? There is such a thing as poor man's gout, and, as Mr. Jonathan Hutchison very properly remarks, there are certain individuals in whom no amount of abuse of either food or alcohol will excite gouty manifestations. The gouty individual is one with a peculiar habit of body, showing itself not only by the deposition of uric acid in certain tissues, but by modifying almost every function of the body. This habit is accompanied frequently by high arterial tension, and is followed in course of time by degeneration of the kidney and blood-vessels. Is this complex of phenomena traceable to any external agent such as high living? Or is the external agent simply one means of rendering the inherent vice apparent? The presence of uric acid in the blood and tissues has been held by Garrod to be the diagnostic feature of the disease, and this formation of uric acid has by some been looked upon as evidence of sub-oxidation of nitrogenous waste. We know, however, that reptiles and birds excrete a large part of their nitrogen in the shape of uric acid. May we not entertain, therefore, as a possibility, that the gouty constitution, so-called, is in part a reversion to some far back ancestor in which uric acid was excreted normally to a much larger extent than it is at present in an average member of the human

race? It has always seemed to me that one great reason why it tends to be deposited in the tissues is the fact that the kidney, owing to its degeneration, fails to excrete it.

The conclusion I have arrived at as regards the origin and heredity of this disease is very much the same as that bearing upon tuberculosis, namely, that the gouty habit of body has arisen as a variation, and as such is hereditarily transmissible, and that excess of diet and alcohol merely render the habit of body apparent.

I come next to the great class of mental diseases, those diseases which manifest themselves in connection with what is termed the neurotic or the psychopathic constitution. In approaching this subject, it is to be borne in mind that habitude, sensation, perception, association of sensations, and images, in one word, the modality of the spirit, are bound up indissolubly with organic substrata, and as these are hereditary, it follows that intellectual aptitudes are so also. As Debierre puts it—"Man thinks and acts, not spontaneously, but according to the blood which is in his veins; that is to say, according to his heredity. He thinks, he feels, he wills much more through his ancestors than through himself." Thought is nothing more than a secretion, the result of a certain metabolism; and it stands to reason that from time to time, in the phylogenetic history of a nervous system so complex as that of man, variations in the durability and resistance of the mechanism underlying this metabolism must have taken place, as we know to have occurred in the internal mechanism of other organs of the body. Mental derangements, we know, are among the commonest of diseases. It may be that the individual is not so far deranged as to require restraint. There are minor degrees of mental derangement which may be included under the terms wrong-headed, eccentric, fanatical, or hypochondriacal, which may manifest themselves in a line of psychopathic individuals, which are quite distinctive of the type, and which may break out at a certain period, and in a particular member of the family, in one or other of the various definite modes of what is commonly termed mental alienation. All such manifestations indicate a certain diminished resistance or power of endurance on the part of the nerve-cells of the brain; and in this relationship, and in reference to what I have indicated as my own persuasion of the essential element in the heredity of the tubercular constitution, it must be borne in mind that nerve cells are of epidermic origin, and that the tubercular constitution is frequently associated with the psychopathic. I have attempted to trace the predisposition in tuberculosis to a want of resistance on the part of the epithelia to the encroachments of the tubercle bacillus. May not there be a certain connection between the two classes of disease in respect of their representing in reality the same hereditary peculiarity, the same dyscrasia, the same tendency to decay on the part of the nerve-cells and epithelial-cells?

I have often thought that another factor predisposing to mental derangement is that nerve-cells show so little capability, if any at all, of regeneration. We are born apparently with the nerve-cells which will serve us throughout life. Is it a matter of wonder that frequently these break down under unusual strain, or what is probably much more likely as an inherited peculiarity? The germ-track followed in the ontogeny of the nerve-cells is very short, far shorter than in the case of many other cells throughout the body, and hence a state of maturity is reached at a comparatively early period, with an inclination to premature decay

But in this class of diseases, as in others which may be considered in the ordinary acceptation of the term to be hereditary, the great question continues to assert itself, namely, whether external agencies can bring about a state of the nervous system which is distinctly morbid, and which can be transmitted for generations in the offspring. The evidence in support of the positive view of this question which is usually quoted is that of the production and hereditary transmission of epilepsy in guinea-pigs. Brown-Séquard's experiments appeared to show that after hemisection of the cord or division of the great sympathetic in the neck, not only did the animals become epileptic, that is to say, not only could they be readily thrown into a convulsive fit, but that this peculiarity of constitution thus engendered could be transmitted to their young. The operation on the sympathetic brings about atrophic lesion of the eyeball which, it was asserted, is also transmitted. I must confess it has always seemed to me that an element of fallacy has entered into these experiments which would require to be eradicated before we can found any conclusions upon them. Have we crucial evidence to show that a mental disease may be excited through external agencies, as for instance, by the abuse of alcohol in a person free from any ancestral taint, and that this disease so excited can be transmitted through several generations. My own impression is that we have not; but it would be a matter of extreme interest to me, as I daresay to other members of this Society, to hear of any confirmation of the allegation. So far as I am personally informed, I feel that in mental derangement and in excess of perhaps any other form of disease we have to do with an inherited peculiarity or variation, a variation which may have occurred in a far back ancestor and lain dormant for many generations, but which inevitably manifests itself under conditions of unusual external stimulation, and which is in no respect bound up etiologically or necessitated by this stimulus. The substratum which underlies the mental peculiarity is allied to that underlying the predisposition to tuberculosis or gout, and probably is referable to a fault in metabolism excited, it may be, by an inherent bias towards degeneration in the nerve-cells of the brain, and this is eminently hereditary.

From what I have already said it will be apparent that I do not regard syphilis as a hereditary disease. Congenital it certainly is, but I fail to note that, once acquired, it can be transmitted as a truly hereditary affection. As in the case of most contagious diseases, the contagion can pass from mother or father to child, but has an inevitable tendency to die out in the early years of life in the first descendant. This is not heredity but intra-uterine contagion.

Hæmophilia appears to be in many instances a truly heritable disease, and so far as our lights go in pointing its pathology, they seem to indicate that it is more a disease of the blood-vessels than one of the blood. The vessels do not seem to contract as they ought to do, and their coats are peculiarly friable. One could quite well understand this peculiarity being transmitted, just as the tendency to hæmorrhoids is said to be transmissible on account of the veins concerned having unusually thin walls. I have often thought, however, that there is something more in hæmophilia than this mere anatomical peculiarity, and that there may be a close relationship between it and menstruation. The fact that it is transmitted in the male line is significant. It has often been alleged that menstruation occurs in the male as well as the female; that although there is no actual discharge

of blood, there exists the monthly constitutional disturbance of the general functions in the male as well as in the female, accompanied, it may be, by rise of temperature, a tendency to undue relaxation of the blood-vessels supplying the organs of generation, and possibly also of the hæmorrhoidal vessels. There seems to me to be a good deal of truth in this allegation. May not hæmophilia be simply a manifestation of this menstruation in the male?

Many more examples might be brought forward illustrative of the thesis I have started with in my remarks, but the above will probably suffice to elicit the opinions of others.

Turning now to another matter, I would ask the attention of the meeting to the consideration of what evidence we have of a hereditary tendency to disease becoming eradicated by inbreeding with individuals having no such tendency. How soon can we assert that the hereditary psychopathic constitution becomes eradicated in the descendants under such circumstances? Or, does it become eradicated at all? If we accept the statement that the extrusion of the polar cells with their quota of chromosomes before fertilisation occurs represents a true reduction process, that at this time a certain part of the maternal heredity is got rid of by this means, have we any reason for believing that in course of time a hereditary vice of constitution may entirely disappear? And here the facts of atavism come in to our aid. Polydactylism in man is supposed to be a reversion to a heptadactylous ancestor; the striping of mules is said to point back to an early equine ancestor, and horses and asses are said to contain solitary "zebra" determinants in their germ-plasm; the three-toed horse anomaly is alleged to be a reversion to the fossil hipparion of the Eocene period; the lanugo of the fœtus, a reversion to a hair-clad ancestor. I am told by a breeder of black polled Angus cattle that the progeny of a perfectly black polled bull and cow is sometimes a red calf with horns; indeed, unless the pedigree of mother and sire is pure for generations back, that no reliance can be placed upon the progeny they may beget. If these statements be true, what are we to say of the transmission of variations in constitution? My firm conviction is that if a vicious line is introduced it may die out, and probably does in most cases die out by interbreeding with a series of pure stocks, but that no reliance can be placed upon its not recurring atavistically, it may be, generations afterwards.

Lastly, let me refer briefly—and it must be briefly, for I am afraid I have already exceeded the limit of time put at my disposal—to the very interesting subjects of maternal impressions and telegony in disease. The influence of the mother upon the fœtus *in utero* must be very great, perhaps far greater than is generally admitted, in so far at least as nutritional factors are concerned. The subject of maternal impressions, however, is open to many fallacies, mere coincidences being interpreted as evidence bearing upon the matter. For my own part, I have little belief in maternal impressions as a cause of malformation or other peculiarity. As an example of the sort of evidence usually brought forward in support of the theory, I may refer to the case mentioned by Hippocrates of the white woman who bore to her white husband a black child, and who explained the occurrence by there having hung in her bedroom a picture of a black man—and so on it goes. Have we any proof of the occurrence of telegony in disease? From the evidence collected by Romanes and others, there seems to be no doubt of the mother being impressed by the male element

of a first pregnancy in such fashion that the subsequent progeny by another male partake of the features of the father of the first born, and I daresay Professor Cossar Ewart may have something interesting to tell us on this point. But what I would endeavour to elicit from the discussion this evening in this relationship is whether we have anything analogous in the transmission of hereditary diseased states of body. Are there any grounds for believing, for instance, that a father with a strong tubercular hereditary history, and bearing the lineaments of such in his person, can so affect the system of a healthy mother that the children subsequently born to the same woman by a second husband, with no such strain in his blood and no such lineaments, have the tubercular characteristics of the first husband impressed upon them? I know of no evidence bearing on the problem, but possibly some members of the Society here this evening can throw light upon it.

In conclusion, allow me to formulate the substance of my remarks in the following synopsis:—

(1.) There is no evidence proving that diseased conditions of body, excited by external agencies, using the term in its broadest sense, can be transmitted hereditarily through generations.

(2.) That the various hereditary tendencies or predispositions to disease of the hereditary type have arisen as variations in the germ-plasm.

(3.) That these predispositions to disease probably extend far back into the history of the human race, and break out only occasionally, in accordance with the laws of atavism.

(4.) That external agencies are merely the means of bringing them to light.

(5.) That there is little if any reliance to be placed in the evidence bearing upon the influence of maternal impressions.

(6.) That there is no reason to believe that telegony may not prevail in the case of hereditary predisposition to disease as it evidently does in regard to other characteristics.

FACTS OF INHERITANCE.[1]

BY PROFESSOR J. ARTHUR THOMSON, M.A.

ONE of the distinctive features of the nineteenth century has been a reduction in the number of supposed separate powers or entities—the use of William of Occam's razor, in fact. "Caloric" was one of the first to be eliminated, yielding to the modern interpretation of "heat as a mode of motion"; "Light" had to follow, when the undulatory theory of its nature was accepted; a specific "Vital Force" is disowned even by the Neo-vitalists; "Force" itself has become a mere measure of motion; and so on. In view of this progress towards greater precision of phraseology, it cannot be a matter for surprise that a biologist should affirm that to speak of the "Principle of Heredity" in organisms is like speaking of the "Principle of Horology" in clocks. The sooner we get rid of such verbiage the better for clear thinking, since heredity is certainly no power or force or principle, but a convenient term for the relation of organic or

[1] Address delivered at the Royal Institution of Great Britain, 30th March 1900.

genetic continuity which binds generation to generation. Ancestors, grandparents, parents are real enough; children and children's children are also very real; heredity is a term for the relation of genetic continuity which binds them together. As for such a question as this, "Is my grandfather's environment my heredity?" it is an offence against Queen's English as much as against scientific phraseology; it should probably read, "Have the structural changes induced by external stimulus on my grandfather's body had any effect on my inheritance?"

Another distinctive feature in scientific progress has been the introduction of precise measurement. It is hardly too much to say that in the development of natural knowledge, science begins where measurement begins. And this is the case in regard to inheritance, As long as we are content to say, "This child takes after his grandfather," "This pigeon shows a throw-back to its rock-dove ancestry," and so on, we may be making interesting remarks, but it is only when we are able to give precise measurements of the amounts of resemblance or difference that we make contributions of real importance to that department of life-lore which deals with inheritance. Or perhaps, instead of measurement, which may be taken in too narrow a sense, I should say that precision of observation and record which admits of statistical, mathematical, or some other exact formulation. While nothing can take the place of experiment—which is urgently needed for the further development of our knowledge of heredity—much has been gained by the application of statistical and mathematical methods to biological results—a new contact between different disciplines—which we may particularly associate with the names of Mr. Francis Galton and Mr. Karl Pearson.

I.—The Physical Basis of Inheritance.

What was for so long quite hidden from inquiring minds, or but dimly discerned by a few, is now one of the most marvellous of biological commonplaces—that the individual life of the great majority of plants and animals begins in the union of two minute elements—the sperm-cell and the egg-cell. These microscopic individualities unite to form a new individuality, a potential offspring which will by and by become like to, and not different from, its parents. If we mean by inheritance to include all that the living creature is or has to start with in virtue of its genetic relation to its parents and ancestors, then it is plain that the physical basis of inheritance is in the fertilised ovum. As regards property, there is an obvious distinction between the inheritance and the person who inherits, but there is no such distinction in biology. The fertilised egg-cell *is* the inheritance, and is at the same time the potential inheritor. What might be compared to an inheritance of property as apart from the organism itself is the store of food which may be inside the egg, or round about it.

An organic inheritance means so much, even when we use the magic word potentiality, that although we are quite sure that the germ-cells constitute the physical basis of inheritance, we may consider for a moment the difficulty which rises in the minds of many when they remember that the egg-cell is often microscopic, and the sperm-cell often only $\frac{1}{100000}$th of the ovum's size. Can there be room, so to speak, in these minute elements for the complexity of organisation supposed to be requisite? And the difficulty will be increased if the current opinion be accepted that only the

nuclei within the germ-cells are the true bearers of the hereditary qualities. Darwin spoke of the pinhead-like brain of the ant as the most marvellous little piece of matter in the world, but must we not rank as a greater marvel the microscopic germ-cells which contained potentially all the inherited qualities of that ant, or of that man?

Nowhere more than in biology is one made to feel that a little may go a long way. A microbic spore invisible even with a fairly good microscope may kill a man. From one microscopic egg of a sea-urchin cut into three, Delage reared three larvæ. In another case he says that he reared an embryo from a $\frac{1}{31}$th fraction of an egg. We know of twin animals developed from one egg, but what shall we say of the quadruplets Wilson obtained by shaking apart the four-cell stage in the development of the lancelet, or of the "legion of embryos," which Marchal describes as developing from a single ovum of a peculiar Hymenopterous insect, *Encyrtus*. In development, indeed, a half may be as good as a whole.

In reference to the difficulty raised in some minds by the minuteness of the physical basis, it may be recalled that the students of physics, who make theories regarding the sizes of atoms and molecules, which they have invented, tell us that the image of a "Great Eastern" filled with framework as intricate as that of the daintiest watches does not exaggerate the possibilities of molecular complexity in a spermatozoon, whose actual size may be less than the smallest dot on the watch's face. Secondly, as we learn from embryology that one step conditions the next and that one structure grows out of another, we are not forced to stock the microscopic germ-cells with more than initiatives. Thirdly, we must remember that development implies an interaction between the growing organism and a complex environment without which the inheritance would remain unexpressed, and that the full-grown organism includes much that was not inherited at all, but has been acquired as the result of nurture or external influence.

In the performationist theories, which held sway in the seventeenth and eighteen centuries—theories which asserted the pre-existence of the organism and all its parts, in miniature, within the germ—there was a kernel of truth well concealed within a thick husk of error. For we may still say, as the performatists did, that the whole future organism is implicit in the germ, and that the germ contains not only the rudiment of the adult organism, but the potentiality of successive generations as well. But what baffled the earlier investigators was the question how the germ-cell comes to have this ready-made organisation, this marvellous potentiality. Discovering no natural way of accounting for this, the majority fell back upon a hypothesis of hyperphysical agencies, that is to say, they abandoned the scientific method, and drew cheques upon that bank where credit is unlimited as long as credulity endures.

An attempt to solve the difficulty which confronted the performationists —the difficulty of accounting for the complex organisation presumed to exist in the germ-cell—is expressed in a theory which seems to have recurred at intervals in the long period between Democritus and Darwin, the theory of pangenesis. On this theory the cells of the body are supposed to give off characteristic and representative gemmules, these are supposed to find their way to the reproductive elements, which thus come to contain, as it were, concentrated samples of the different components of the body, and are therefore able to develop into an offspring like the parent. The theory involves many hypotheses, and is avowedly unverifiable in

direct sense-experience, but the same might be said about many other theories. It is perhaps more to the point to notice that there is another theory of heredity which is, on the whole, simpler, which seems, on the whole, to fit the facts better, especially the fact that our experience does not warrant the conclusion that the modifications or acquired characters of the body of the parent affect in any specific and representative way the inheritance of the offspring.

As is well known, the view which many, if not most, biologists now take of the uniqueness of the germ-cells is rather different from that of pangenesis. It is expressed in the phrase "germinal continuity," and has been independently suggested by several thinkers, though Weissmann has the credit of working it out into a theory. Let me recall its purport. There is a sense, as Mr. Galton says, in which the child is as old as the parent, for when the parent's body is developing from the fertilised ovum, a residue of unaltered germinal material is kept apart to form the future reproductive cells, one of which may become the starting-point of a child. In many cases, scattered through the animal kingdom, from worms to fishes, the beginning of the lineage of germ-cells is demonstrable in very early stages before the differentiation of the body-cells has more than begun. In the development of the threadworm of the horse, according to Boveri, the very first cleavage divides the fertilised ovum into two cells, one of which is the ancestor of all the body-cells, and the other the ancestor of all the germ-cells. In other cases, particularly among plants, the segregation of germ-cells is not demonstrable until a relatively late stage. Weissmann, generalising from cases where it seems to be visibly demonstrable, maintains that in all cases the germinal material which starts an offspring owes its virtue to being materially continuous with the germinal material from which the parent or parents arose. But it is not on a continuous lineage of recognisable germ-cells that Weissmann insists, for this is often unrecognisable, but on the continuity of the germ-plasm—that is, of a specific nuclear substance of definite chemical and molecular structure which is the bearer of the hereditary qualities. In development, a part of the germ-plasm "contained in the parent egg-cell is not used up in the construction of the body of the offspring, but is reserved unchanged for the formation of the germ-cells of the following generation." Thus the parent is rather the trustee of the germ-plasm than the producer of the child. In a new sense, the child is a chip of the old block. Early segregation of the germ-cells is in many cases an observable fact—and doubtless the list of such cases will be added to—the conception of a germ-plasm is hypothetical, just as the conception of a specific living stuff or protoplasm is hypothetical. In the complex microcosm of the cell, we cannot point to any one stuff and say "this is protoplasm;" it may well be that vital activity depends upon several complex stuffs which, like the members of a carefully constituted firm, are characteristically powerful only in their inter-relations. In the same way, it must be clearly understood that we cannot demonstrate the germ-plasm, even if we may assume that it has its physical basis in the stainable nuclear bodies or chromosomes. The theory has to be judged, like all conceptual formulæ, by its adequacy in fitting facts.

Let us suppose that the fertilised ovum has certain qualities, *a*, *b*, *c*, . . . *x*, *y*, *z*; it divides and re-divides, and a body is built up; the cells of this body exhibit division of labour and differentiation, losing their likeness to the ovum and to the first results of its cleavage. In some of the body-cells

the qualities *a*, *b* find predominant expression, in others the qualities *y*, *z*, and so on. But if, meanwhile, there be certain germ-cells which do not differentiate, which retain the qualities *a*, *b*, *c*, . . . *x*, *y*, *z*, unaltered, which keep up, as one may say figuratively, "the protoplasmic tradition," these will be in a position by and by to develop into an organism like that which bears them. Similar material to start with, similar conditions in which to develop, *therefore*, like tends to beget like.

May we think for a moment of a baker who has a very precious kind of leaven, and some less precious material to work with; he uses much of this in baking a large loaf, but he so arranges matter by a clever contrivance that part of the original leaven is always carried on unaltered, carefully preserved for the next baking. Nature is the baker, the loaf is a body, the leaven is the germ-plasm, and each baking is a generation.

II.—Dual Nature of Inheritance.

Apart from exceptional cases, the inheritance of a multicellular animal or plant is dual, part of it comes from the mother and part of it from the father; the beginning of the new individuality is a fertilised egg-cell. The exceptions referred to are cases of asexual multiplication by buds or otherwise, as in the freshwater Hydra; cases of parthenogenesis, as in the case of the unfertilised eggs which develop into green fly in the summer, and cases like liver-flukes, where an animal is both mother and father to its offspring. Apart from these exceptions, the inheritance does at the start consist of maternal and paternal contributions in intimate and orderly union.

Professor E. B. Wilson states the general opinion of experts somewhat as follows:—As the ovum is much the larger, it is believed to furnish the initial capital—including it may be a legacy of food-yolk—for the early development of the embryo. From both parents alike comes the inherited organisation which has its seat (according to many) in the readily stainable (chromatin) rods of the nuclei. From the father comes a little body (the centrosome) which organises the machinery of division by which the egg splits up, and distributes the dual inheritance equally between the daughter cells.

Recent discoveries have shown that the paternal and maternal contri butions which come together in fertilisation are for several divisions at least exactly divided among the daughter cells, thus confirming a prophecy which Huxley made in 1878: "It is conceivable and indeed probable that every part of the adult contains molecules derived both from the male and from the female parent; and that, regarded as a mass of molecules, the entire organism may be compared to a web of which the warp is derived from the female and the woof from the male." "What has since been gained," says Professor Wilson, "is the knowledge that this web is to be sought in the chromatic substance of the nuclei, and that the centrosome is the weaver at the loom."

In regard to these conclusions I wish to make three remarks. (*a*) Although inheritance is dual, it is in quite as real a sense multiple, from ancestors through parents, as we shall afterwards see. (*b*) If Loeb is able to induce artificial parthenogenesis in sea-urchins' eggs exposed for a couple of hours to sea-water to which some magnesium chloride has been added; if Delage is able to fertilise and to rear normal larvæ from non-nucleated

ovum fragments of sea-urchin, worm and mollusc, we should be chary in committing ourselves definitely to the conclusion that the nuclei are the exclusive bearers of the hereditary qualities, or that both must be present in all cases. Furthermore, the fact that an ovum without any sperm-nucleus, or an ovum fragment without any but a sperm-nucleus, can develop into a normal larva, points to the otherwise probable conclusion that each germ-cell, whether ovum or spermatozoon, bears a complete equipment of hereditary qualities. (*c*) It must be carefully observed that our second fact does not imply that the dual nature of inheritance must be patent in the full-grown offspring, for hereditary resemblance is often strangely unilateral, the characters of one parent being "prepotent," as we say, over those of another.

III.—Different Degrees of Hereditary Resemblance.

Before the middle of the century considerable attention was paid to what might be called the demonstration of the general fact of inheritance. In a big treatise like that of Prosper Lucas (1847) many hundreds of pages are devoted to proving what we now take for granted—that the present is the child of the past, that our start in life is no haphazard affair but is rigorously determined by our parents and ancestors, that various peculiarities, normal and abnormal, physical and mental, may reappear generation after generation, and so on. One step of progress during the Darwinian era has been the recognition of inheritance as a fact of life which requires no further proof.

Yet this aspect of the study of heredity is by no means worked out. Thus there are some characters, *e.g.*, tendencies to certain diseased conditions, which are more frequently transmitted than others, and we ought to have, in each case, precise statistics as to the probabilities of transmission.

Again, there are some subtle qualities whose heritability must not be assumed without evidence. Thus it is of very great importance to students of organic evolution that Professor Karl Pearson has recently supplied, for certain cases, definite proof of the inheritance of fecundity, fertility, and longevity.

The familiar saying, "like begets like," should rather read, "like tends to beget like," since variation is quite as important a fact as complete hereditary resemblance. If it seems to us that in many cases the offspring is practically always a facsimile reproduction of the parent, this may be due to absence of variation, or, what comes almost to the same thing, to great completeness of inheritance; but it is more likely to be due to our ignorance, to our inability to detect the idiosyncrasies.

But it will be granted by all that the completeness with which the characters of race, genus, species, and stock are reproduced generation after generation, is one of the large facts of inheritance. It is obvious, however, that this does not sum up our experience, and we must face the task of considering what may be called the different degrees of hereditary resemblance. For these a confused classification and a troublesome terminology has been suggested, to discuss which would be most unprofitable in the limits of a short lecture.

I therefore propose to restrict attention to three familiar cases, which are called blended, exclusive, and particulate inheritance, and then to say a few words in regard to the phenomena known as regression.

A preliminary consideration must be attended to. It is a matter of

observation that there are great differences in the degree in which offspring resemble their parents; but it is surely a matter of conjecture that lack of resemblance is necessarily due to incompleteness in the inheritance. Indeed, the fact that the resemblance so often reappears in the third generation, makes it probable that the incompleteness is not in the inheritance, but simply in its expression. The characters which seem to be absent, to "skip a generation," as we say, are probably part of the inheritance, as usual. But they remain latent, neutralised, silenced (we can only use metaphors) by other characters, or else unexpressed because of the absence of the appropriate stimulus.

We can imagine the son of a lavish millionaire reacting to plain living; we can imagine the superficial supposition that the money had been lost; and we can imagine the complete contradiction of this inference in the third generation.

(*a*) In *blended* inheritance, the characters of the two parents, *e.g.*, in regard to particular structure, such as the colour of the hair, may be intimately combined in the offspring. This is particularly well seen in some hybrids, where the offspring seems like the mean of the two parents; it is probably the most frequent mode of inheritance.

(*b*) In *exclusive* inheritance, the expression of maternal or of paternal characters in relation to a given structure, such as eye-colour, is suppressed, Sometimes the unilateral resemblance is very pronounced, and we say that the boy is "the very image of his father," or the daughter "her mother over again"; though even more frequently the resemblance seems "crossed," the son taking after the mother, and the daughter after the father. Our emphasis on the distinction between inheritance and the expression of inheritance is surely warranted by cases on record where the young boy resembled the mother and the girl the father; but when they came of age, the likeness was reversed, *i.e.*, formerly obscure resemblances became dominant.

(*c*) It seems convenient to have a third category for cases where there is neither blending nor exclusiveness, but where in the expression of a given character, part is wholly paternal and part wholly maternal. This is called *particulate* inheritance. Thus, an English sheep-dog may have a paternal eye on one side and a maternal eye on the other. Suppose the parents of a foal to be markedly light and dark in colour; if the foal is light brown, the inheritance in that respect is blended, if light or dark it is exclusive, if piebald it is particulate. In the last case there is in the same character an exclusive inheritance from both parents.

The numerous experiments on hybridisation made by botanists, zoologists, and more practical people, have led us to expect one of three results when a crossing has a successful issue:—(1) The hybrid may be intermediate between its parents, sometimes so exactly that we may liken the blending, not merely to warp and woof, but to a mingling of two colours; (2) the hybrid may show an exaggeration of the characters of one parent, often with little apparent realisation of the peculiarities of the other. These correspond to blended and exclusive inheritance in ordinary cases of mating between the same breed. But (3) the hybrid may also be very different from either parent, showing features which appear to be quite novel, or which on close investigation are seen to be interpretable as the reassertion of the characters of a remoter ancestor. In short, it may show either a new variation or a reversion. The extraordinary fact is that

at least two of these different results may be illustrated in one brood or litter of hybrids.

The facts above referred to may be considered in another aspect, in terms of what is called the quality of prepotency, with which breeders have been for a long time familiar. The term refers to the fact that in the development of a character the paternal or the maternal qualities may predominate, as in unequal blending where there is relative prepotency, or in exclusive inheritance where the prepotency in respect to a given character is absolute. It seems doubtful whether we gain much by using the word, since all these general terms are apt to form the dust particles of intellectual fog, but what we have to do with is the fact that in respect to certain characters the paternal inheritance seems more potent than the maternal, or *vice versa*. Thus in man the father is usually prepotent in the matter of stature, and breeders give many instances where certain, even trivial, characters of sire or dam appear persistently in the offspring, irrespective of the nature of the other parent.

It seems that one of the ways in which the quality of prepotency may be developed is by inbreeding, as Professor Ewart and others have maintained. "Some breeders say that they can produce a horse so prepotent, so fixed by interbreeding (inbreeding), that it will produce its like however mated;" and there is much evidence to show that, of two parents, the more inbred—up to a certain limit of stability—is likely to have the greater influence on the offspring.

As inbreeding may be frequent in nature, especially among gregarious and isolated groups, and as it tends to develop prepotency, we are able to understand better how new variations may have been fixed in the course of evolution. And we can better understand the position maintained by Reibmayr, that the evolution of a human race implies alternating periods of dominant inbreeding and dominant cross-breeding. The inbreeding gives fixity to character, the cross-breeding averts degeneracy and stimulates new variations which form the raw material of progress. The Jews, especially in isolated colonies, may serve to illustrate persistent inbreeding, which we may contrast with the complex cross-breeding at present conspicuous in America.

Until we have more precise statistical data in regard to blended, exclusive, and particulate inheritance, we cannot hope to simplify the matter with any security. But perhaps a unified view will be found in the theoretical conception of a germinal struggle in the arcana of the fertilised ovum, a struggle in which the maternal and paternal contributions may blend and harmonise, or may neutralise one another, or in which one may conquer the other, or in which both may persist without combining. We have extended the wide conception of the struggle for existence in many directions; it may be between organisms akin or not akin, between plants and animals, between organisms and their inanimate environment, between the sexes, between the different parts of the body, between the ova, between the spermatozoa, between the ova and the spermatozoa, and Weissmann has suggested that it may also be between the constituents of the germ-plasm.

IV.—Regression.

We have already referred to the fact which stares us in the face, that there is a sensible stability of type from generation to generation. "The large," Mr. Galton says, "do not always beget the large, nor the small the

small; but yet the observed proportion between the large and the small, in each degree of size and in every quality, hardly varies from one generation to another." In other words, there is a tendency to keep up a specific average. This may be partly due to the action of natural elimination, weeding out abnormalities, often before they are born. But it is to be primarily accounted for by what Mr. Galton calls the fact of "filial regression." Let me take an instance from Mr. Pearson's "Grammar of Science." Take fathers of stature 72 inches, the mean height of their sons is 70·8, we have a regression towards the mean of the general population. On the other hand, fathers with a mean height of 66 inches give a group of sons of mean height 68·3 inches, again nearer the mean. "The father with a great excess of the character contributes sons with an excess, but a less excess of it; the father with a great defect of the character contributes sons with a defect, but less of it."

As Mr. Galton puts it, society moves as a vast fraternity. The sustaining of the specific average is certainly not due to each individual leaving his like behind him, for we all know that this is not the case. It is due to a regression which tends to bring the offspring of extraordinary parents nearer the average of the stock. In other words, children tend to differ less from mediocrity than their parents.

This big average fact is to be accounted for in terms of that genetic continuity which makes an inheritance not dual but multiple. "A man," says Mr. Pearson, "is not only the product of his father, but of all his past ancestry, and unless very careful selection has taken place, the mean of that ancestry is probably not far from that of the general population. In the tenth generation a man has [theoretically] 1024 tenth great-grandparents. He is eventually the product of a population of this size, and their mean can hardly differ from that of the general population. It is the heavy weight of this mediocre ancestry which causes the son of an exceptional father to regress towards the general population mean; it is the balance of this sturdy commonplaceness which enables the son of a degenerate father to escape the whole burden of the parental ill."

At this point one should discuss reversion or atavism, but it is exceedingly difficult to get a firm basis of fact. I use the term reversion to include cases where through inheritance there reappears in an individual some character which was not expressed in his parents, but which did occur in an ancestor. I say advisedly "through inheritance," in order to exclude those cases where the reappearance of the character can be accounted for in some other way. The term thus defined is a very wide one, and not very fortunate, but it is difficult to get rid of. I use it to refer to abnormal as well as normal characters, even to include the reappearance of characters, the normal occurrence of which was outside the limits of the race altogether, *i.e.*, in some phyletically older race. In other words, the character whose reappearance is called a reversion may be found within the verifiable family, within the breed, within the species, or even in a presumed ancestral species.[1]

[1] Professor Karl Pearson defines a *reversion* as "the full reappearance in an individual of a character which is recorded to have occurred in a *definite* ancestor of the same race," and *atavism* as "a return of an individual to a character not typical of the race at all, but found in allied races supposed to be related to the evolutionary ancestry of the given races." "In reversion we are considering a variation, normal or abnormal, from the standpoint of *heredity in the individual*; in atavism we are considering an abnormal variation from the standpoint of the *ancestry of the race*." But as the two words seem to be used by some authors in the converse way, or as equivalent, and as it is surely difficult to define the field of abnormal variation, I have adhered to the wider usage.

The best illustrations of reversion are furnished by hybrids. Thus in one of Professor Cossar Ewart's experiments a pure white fantail pigeon, of old-established breed, which in colour had proved itself prepotent over a blue pouter, was mated with a cross previously made between an owl and an archangel, which was far more of an owl than an archangel. The result was a couple of fantail-owl-archangel crosses, one resembling the Shetland rock-pigeon, and the other the blue rock of India. Not only in colour, but in shape, attitude, and movements there was an almost complete reversion to the form which is believed to be ancestral to all the domestic pigeons. The only marked difference was a slight arching of the tail. Similar results were got with fowls and rabbits.

But carefulness is necessary in arguing from the results of hybridisation to those of ordinary mating, and even if some of the phenomena of exclusive inheritance seem to show reversion to a near ancestor we need a broader basis of fact than we have at present before we can formulate any law. It is impossible to read the recorded cases without becoming convinced that many phenomena are labelled reversions on the flimsiest evidence. Thus the occurrence of a Cyclopean human monster with a median eye has been called a reversion to the sea-squirt, and gout has been called a reversion to the reptilian condition of liver and kidneys. Often there is not the slightest attempt to discriminate between true reversion (*i.e.*, the expression of latent ancestral characters) and the phenomena of arrested development, or of abnormalities which have been induced from without. Often, too, there has been no scruple in naming or inventing the ancestor to whom the reversion is supposed to occur, although evidence of the pedigree is awanting; and the vicious circle is not unknown of arguing to the supposed ancestor from the supposed reversion, and then justifying the term reversion from its resemblance to the supposed ancestor. Little allowance has been made for coincidence, and the postulate of characters remaining latent for millions of years is made as glibly as if it were just as conceivable as a throw-back to a great-grandfather.

I do not see any way out of the theory that characters may lie latent for a generation or for generations, or in other words that certain potentialities or initiatives which form part of the heritage may remain unexpressed for lack of the appropriate liberating stimulus, or for other reasons, or may have their normal expression disguised. The drone bee has a mother, the queen, but no father, for the eggs which develop into drones are not fertilised, yet his structure differs from that of the queen in other points besides those immediately related with sex, and he may in his turn be the father of future queens and workers. At the same time it does not follow that the reappearance of an ancestral character not seen in the parents is necessarily due to the reassertion of latent elements in the inheritance. It may be a case of ordinary regression; it may be a case of arrested development; it may be an extreme variation whose resemblance to an ancestral characteristic is a coincidence; it may be an individually acquired modification, reproduced apart from inheritance, by a recurrence of suitable external conditions, and so on. In short, what are called reversions are probably in many cases misinterpretations.

V.—Galton's Law.

The most important general conclusion which has yet been reached in

regard to inheritance is formulated in Galton's Law. Mr. Galton was led to it by his studies on the inheritance of human qualities, and more particularly by a series of studies on Basset hounds. It is one of those general conclusions which have been reached statistically, and I must refer for the evidence and also for its strictest formulation to the revised edition of Mr. Pearson's "Grammar of Science."

As we have seen, it is useful to speak of a heritage as dual, half derived from the father and half from the mother. But the heritable material handed on from each parent was also dual, being derived from the grandparents. And so on, backwards. We thus reach the idea that a heritage is not merely dual, but in a deeper sense multiple.

Though a comparison with the inheritance of property cannot be exact, we may fancy a youth inheriting an estate, in regard to which it might be said that half of it had belonged to his father and half of it to his mother, yet one with a full knowledge of the family history and the gradual acquisition of the property, might be able to make the story of the heritage much more interesting by showing how this meadow was due to a grandmother and that forest to a great-grandfather.

To appreciate the possible complexity of our mosaic inheritance we must recall the number of our ancestors. We have two parents, four grandparents, eight great-grandparents, about sixteen great-great-grandparents, and so on. "If," as Professor Milnes Marshall said, "we allow three generations to a century, there will have been twenty-five since the Norman Invasion, and a man may be descended not merely from one ancestor who came over in 1066, but directly and equally from over sixteen million ancestors who lived at or about that date." But on these theoretical lines the existence of one man to-day would involve the existence of nearly seventy thousand millions of millions of ancestors at the commencement of the Christian era. Which is absurd, for it overlooks the frequent occurrence of close intermarriage, of cousins for instance.

The problem of reduction in the number of ancestors has been very carefully discussed by genealogists like Professor Lorenz and Dr. F. T. Richter, but we should soon lose ourselves in the discussion. We must be content to take one example. Theoretically, Kaiser Wilhelm II. might have had in the direct line the number of ancestors indicated in the upper row of the following scheme; the second row indicates the number actually known, on to the twelfth generation; the third row gives the number of those possible ancestors of whose existence there is deficient record; and the fourth row gives the probable total.

Generations.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.
(1) Theoretical number	2	4	8	16	32	64	128	256	512	1024	2048	4096
(2) Actual number known	2	4	8	14	24	44	74	111	162	206	225	275
(3) Inadequately known	—	—	—	—	—	—	—	5	15	50	117	258
(4) Probable total	—	—	—	—	—	—	—	116	177	256	342	533

According to Galton's law, "the two parents between them contribute *on the average* one-half of each inherited faculty, each of them contributing

one-quarter of it. The four grandparents contribute between them one-quarter, or each of them one-sixteenth, and so on, the sum of the series, $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+$&c., being equal to 1, as it should be. It is a property of this infinite series that each term is equal to the sum of all those that follow:—thus $\frac{1}{2}=\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+$&c.; $\frac{1}{4}=\frac{1}{8}+\frac{1}{16}+$&c., and so on. The prepotencies or subpotencies of particular ancestors, in any given pedigree, are eliminated by a law that deals only with *average* contributions, and the varying prepotencies of sex in respect to different qualities are also presumably eliminated."

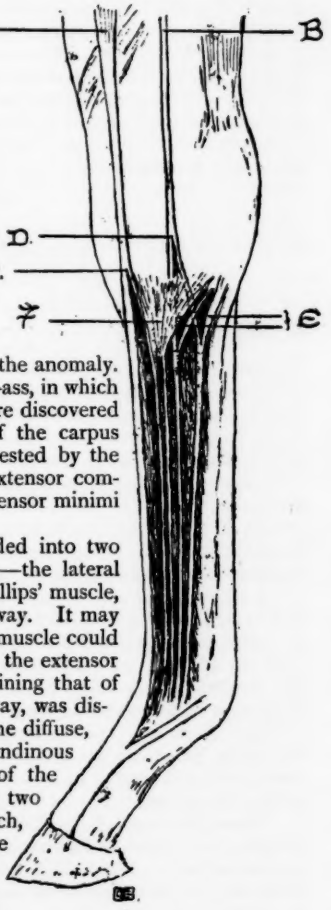
This law of ancestral inheritance, which states that each parent contributes on an average one-quarter, each grandparent one-sixteenth, and so on, rests on researches on human stature, &c., and on colour in Basset hounds, but Professor Karl Pearson trusts it even more because of its success in predicting results. He is very enthusiastic on the subject, and finishes a paper on Galton's law with the following words:—"It is highly probable that it is the simple descriptive statement which brings into a simple focus all the complex lines of hereditary influence. If Darwinian evolution be natural selection combined with *heredity*, then the single statement which embraces the whole field of heredity must prove almost as epoch-making to the biologist as the law of gravitation to the astronomer. Reference should here be made, however, to Mr. Pearson's recent paper (Proc. Roy. Soc., London, March 1900) on the law of reversion.

The aim of this lecture has been to present in brief compass a statement of the leading facts of inheritance, which should be clear in the minds of all. I have said nothing in regard to the transmissibility of acquired characters, for this cannot be ranked at present as an established fact, and I have left some other doubtful points unmentioned. Allow me in conclusion to make this simple remark. The study of inheritance leaves a fatalistic—almost paralysing—impression in many minds, especially perhaps if it be believed that the acquired results of experience and education—of "nurture," in short, cannot be entailed upon the offspring. To some extent this fatalistic impression is justified, but it is well that it should rest upon a sound basis of fact and not on exaggerations. In a sense we can never get away from our inheritance. As Heine said half bitterly, half laughingly, "A man should be very careful in the selection of his parents." On the other hand, although the human organism changes slowly in its heritable organisation, it is very modifiable individually, and "nature" can be bettered by "nurture." If there is little scientific warrant for our being other than sceptical at present as to the inheritance of acquired characters, this scepticism lends greater importance than ever, on the one hand, to a good "nature," to secure which for offspring is part of the problem of careful mating; and, on the other hand, to a good "nurture," to secure which for our children and children's children is one of the most obvious of duties, the hopefulness of the task resting upon the fact that, unlike the beasts that perish, man has a lasting external heritage, capable of endless modification for the better.

AN UNUSUAL MUSCULAR ARRANGEMENT.

BY O. CHARNOCK BRADLEY.

CONTRARY to our usual custom, we recently dissected the body of a donkey in connection with the class of practical anatomy, and found an arrangement of the muscles of the pectoral limb that we have never seen in dissections of horses. In consequence of its strangeness to me, I desire to record the circumstance, remarking, however, that it may be not so very rarely met with in those classes where dissection-subjects are usually of the asinine persuasion. I propose to make no surmises as to the significance of the peculiarity, but merely to describe what we found, and especially to call attention to the fact that the tendons on both sides of the body were arranged in the same way. Those who have a leaning towards speculation may decide for themselves as to the hidden meaning of the anomaly.



A. Extensor communis digitorum.
B. Extensor minimi digiti.
C. Division of tendon of common extensor.
D. Division of tendon of extensor minimi digiti.
E. Slips from pisiform bone.
F. Tendinous expansion connected with Phillips' tendon.

The dissection-subject was a young she-ass, in which no anatomical peculiarities of any kind were discovered until the dissection reached the region of the carpus and metacarpus. Here attention was arrested by the unusual behaviour of the tendons of the extensor communis digitorum (extensor pedis) and extensor minimi digiti (extensor suffraginis).

The extensor communis tendon divided into two unequal parts on a level with the carpus,—the lateral portion being obviously the tendon of Phillips' muscle, leaving the common tendon in the usual way. It may be remarked that a dissection of Phillips' muscle could not be made, as it was firmly blended with the extensor communis. Phillips' tendon, instead of joining that of the extensor minimi digiti in the usual way, was disposed as shown in the sketch. It became diffuse, and formed the border of a triangular tendinous expansion which sprang from the front of the carpus. From the apex of this triangle two small tendons of equal size emerged, which, running down the metacarpal region, were inserted into the base of the first phalanx.

The tendon of the extensor minimi digiti also divided into two portions, these being almost equal in size, and the point of division being at a slightly higher level than that of the tendon of the extensor communis. Both of the tendons so produced were joined by slips springing from the pisiform bone. They remained per-

fectly separate from each other, and had independent insertions to the base of the first phalanx.

I have looked up what literature I could lay hands on, but have failed to find any reference to a condition even remotely similar. The *Annales de Médicine Vétérinaire* for February 1898 contains the translation of a paper by A. Degive, in which various arrangements in this region of the horse's limb are discussed and illustrated, but there is nothing really comparable with what was found in our subject.

I have been moved to record the case because any anomaly of the muscles of the ungulate limb cannot fail to be interesting to the animal morphologist from the circumstance that the limb, especially of solipeds, has undergone so marked a degree of specialisation.

I may add that all the other muscles and tendons conformed to text-book commonplaceness.

STRANGULATED HERNIA: SOME PRACTICAL REMARKS CONCERNING ITS DIAGNOSIS AND ITS PROPER MANAGEMENT.[1]

BY PARKER SYMS, M.D., CLINICAL PROFESSOR OF SURGERY, NEW YORK UNIVERSITY.

STRANGULATED hernia presents too important a subject to be comprehensively set forth in a brief essay, therefore the scope of this paper has been limited to the entirely practical and clinical features. No attempt has been made to minutely describe the pathic anatomy of the condition, nor to review the theories of the etiology of strangulation of a hernia. Broad rules have been treated as being invariable; the exceptions have not been dwelt on.

An incarcerated hernia is one in which the contents of the bowel involved are so included as to prevent their passage or escape, but in which the blood circulation is not materially impeded.

A strangulated hernia, on the other hand, is one in which the constriction is sufficient to shut off the blood circulation of the part involved. For the practical purposes of this paper it may be stated that strangulation of a hernia depends on the fact that the hernial canal or the neck of the sac has become too small for the mass it contains. This may be due to a narrowing of the canal, or to a swelling of the hernia. Volvulus or constriction of the intestine by bands may occur within the sac exactly as they may take place within the abdomen, but in the majority of cases the strangulation is caused by constriction at the hernial canal or at the neck of the hernial sac.

When the return flow of blood is prevented by constriction, the mass becomes congested and swollen; so strangulated hernia is a condition that augments itself. When the blood circulation has been completely arrested by strangulation, gangrene of the part will soon occur. This is so serious a condition that it may be practically considered fatal, and it should never be allowed to take place. The arrangement of the vascular system of the intestine is such that strangulation of even a small loop of bowel results in a serious derangement of the blood supply of the entire intestinal tract.

[1] Read in a Symposium on Strangulated Hernia, before the New York County Medical Association, New York City.

Symptoms and Diagnosis.—The symptoms of strangulated hernia are a logical result of the conditions producing them, and they are absolutely characteristic and distinct, and failure to make a diagnosis is inexcusable. They are those of acute intestinal obstruction with the addition of the local objective and subjective symptoms of the hernia itself. They represent the condition known as abdominal shock followed by intestinal obstruction—later with gangrene of the intestine and general systemic infection. They are in nowise similar to the symptoms of a case of ordinary functional constipation, and it is most important they should be early recognised and properly interpreted, for they indicate a state of affairs which will surely result in death unless spontaneous reduction or surgical interference afford relief.

The prominent symptoms of strangulated hernia are: pain, tenderness, vomiting, obstipation, tympanites, local changes, disturbance of the pulse, disturbance of temperature, facial expression, mental condition, derangement of the capillary circulation.

Pain is usually the first symptom to be noticed. It comes on suddenly, at first radiating from the umbilicus, and finally becoming a general abdominal pain. It is colicky in character and severe in degree. There is also local pain about the hernia itself. Tenderness accompanies the pain and is of a corresponding character. Pain and tenderness occur soon after the strangulation, and they continue till reduction is accomplished or until gangrene has taken place.

Sudden cessation of local pain and tenderness without reduction of the hernia indicate the occurrence of gangrene, and should be regarded as a grave sign of danger.

Vomiting is usually an early and persistent symptom. At first it is reflex in character, being caused by nerve irritation. Later it becomes projectile in character, and then it is due to obstruction of the intestine and to waves of peristaltic action. At first the vomit consists of the contents of the stomach, later of gastric mucus and bile, and finally the contents of the small intestine; then it has the characteristic appearance and odour of stercoraceous vomit.

Obstipation will be present from the first to last, though there may be a fæcal movement from the lower bowel. Aside from this there will be no passage of fæces or gas.

Tympanites will increase till relief is obtained.

The local symptoms, besides those peculiar to hernia, will be those of inflammation, viz., swelling, heat, redness, pain, and tenderness. To these will be added loss of impulse on coughing, and gradual loss of tympanitic resonance.

The pulse is always disturbed in its action, both as regards its frequency and its volume. Its rate is increased and it becomes irregular, small, and thready. As exhaustion takes place the pulse will show evidence of final heart failure.

The temper is variously disturbed. It is often subnormal from shock. When systemic infection has taken place the temperature will be elevated; and finally it will again become subnormal from exhaustion and collapse.

The facial expression is indicative of severe injury or shock. It has the pinched, drawn, anxious, ashen look known as the Hippocratic face.

The mental condition is that found accompanying any severe abdominal injury. The patient exhibits a queer combination of fear and of bragga-

docio, both evincing anxiety about himself and boasting that he feels perfectly well. This often seems like an ill-defined form of delirium. The capillary circulation is always disturbed, as is evidenced by pallor, and by the slight cyanosis of the extremities.

Shock is always present to a greater or lesser degree, and this alone stamps the case as different from one of functional constipation or intestinal derangement, and should lead to a proper diagnosis. It is evidenced by the characteristic pulse, facial expression, pallor and prostration, with the peculiar mental condition described above.

The clinical history of a typical case will be about as follows: The patient who has a hernia—either recently acquired or of long standing—will be suddenly attacked with severe abdominal pain radiating from the umbilicus. Vomiting from nerve reflex may come on as one of the earliest symptoms. From the onset of the attack the patient will show the symptoms of shock. His pulse will be rapid, small, and thready. His surface will be pale and ashen, with slight cyanosis of the finger tips. The face will appear pale, drawn, pinched, and anxious. There will be pain and tenderness in the region of the hernia. The hernia will become swollen, tense, and inelastic. It will not give an impulse on coughing. Constipation will be present from the onset, though there may be a discharge from the lower bowel. No gas will be expelled, and the abdomen will become more and more distended. The patient will be greatly prostrated from the beginning, out of proportion to the other symptoms, and if the strangulation is allowed to continue he will pass from a state of exhaustion to one of final collapse. The temperature will vary according to the conditions described above, and it is not an important element of this disease. The vomiting will sometimes be constant, and sometimes it will cease after the first few hours, but when the obstruction has lasted long the vomiting will recur, and it will be projectile and of the stercoraceous variety. The tongue will be dry and coated, and the breath will be offensive. Hiccough may come on at any time or it may be absent. Spontaneous reduction, surgical relief, or death, will terminate the case.

If one will consider the mechanics of the condition he will see that the strangulated intestine is in the same plight as a limb or finger is with a tight elastic band around it. The blood circulation is cut off by compression, the resultant swelling causing increasing compression, and it will be readily understood that if relief by mechanical means is neglected, death of the part will ensue. The rule has so few exceptions that it may be stated that strangulated hernia unrelieved is always a fatal condition. Fortunately, the converse, that strangulated hernia properly relieved at a sufficiently early date should never be fatal, is true. The danger lies in delay, and when we take up the question of treatment this is one thing I want especially to urge on all: strangulated hernia should be early recognised and promptly relieved.

Treatment.—We come now to the question of treatment, the rules for which are very simple. Taxis should never be employed when it is possible to operate. All that should be done in the way of expectant treatment is to place the patient in a position which most favours spontaneous reduction of the hernia, to apply cold by means of an ice-bag, or cracked ice wrapped up in cloths, to withhold all food from the patient until his condition is relieved, and if these measures do not succeed in accelerating a spontaneous reduction of the hernia, operation should be resorted to before

any of the more severe symptoms have set in. That is to say, one should never wait for fæcal vomiting, nor until the patient is in a condition of exhaustion. In fact, one should not expect a spontaneous recovery from this condition, for spontaneous recovery is the rare exception, and an operation done on a patient who might have recovered without it will have done no harm if it is properly performed, but a patient who requires operation for this condition will surely die if the operation is not done. The reason that such a large mortality is recorded against surgery of strangulated hernia is entirely owing to the fact that the cases are not properly treated, as far as this very point is concerned. Too often do we find that a patient has been subjected to taxis. The manipulations, no matter how gentle, are dangerous, and in many cases have hastened the fatal result. In no case can taxis be regarded free from danger.

Should operation be found necessary, it should be done in the simplest manner possible. As most of these cases are suffering from vomiting, due to nerve reflex or to obstruction of the intestinal canal, there may be risk in administering a general anæsthetic. Where vomiting is persistent and frequent, local anæsthesia should be depended on, for we all recognise the danger of suffocation from administering ether or chloroform to a patient in this condition. Wherefore, it is my rule to administer a full dose of morphia and an ounce of whisky, if the patient be an adult, and then to use cocaine locally, for which purpose I employ a weak solution—from ·1 to ·25 of 1 per cent. The usual preparation of the operation field, as regards asepsis, must be rigidly carried out. After making the incision through the tissues down to the sac, it will become necessary to proceed slowly and cautiously so as to avoid wounding the intestine, which may be adherent to the hernial sac. The incision in these cases should be a free one, permitting thorough exposure of the hernial mass for its inspection and treatment. After opening the hernial sac the band of constriction should be sought with the finger, and, either by dissection or by simple incision, guarded by special director, the constricting portion of the canal or sac should be divided so as to be fully relieved. As strangulation of the intestine interferes so radically with its circulation, it will be found, a very few hours after the onset in some cases, that the intestine may have become gangrenous. In all cases it will be darkly congested with ecchymotic spots, and in every case judgment will be required to determine whether the intestine is in a condition which will render it safe to return it to the abdomen. If it is gangrenous, then it must not be returned, but must be properly dealt with, while if there is doubt as to whether it is gangrenous or viable after relieving the constriction so that the blood circulated may be restored, one should often wait a long time to determine the question, for it is a fact that an intestine which will be apparently dead will after a time show that its life is intact. This will be evidenced by the appearance of fresh blood in the vessels, and finally the restoration to its natural colour.

When the intestine is not gangrenous, after being thoroughly cleansed and inspected to see that there is no spot of perforation, and after the operator has assured himself that there is no cause for strangulation remaining, the loop of it should be returned to the abdomen, and if the patient's condition warrants the slight expenditure of time, one of the standard methods for radical cure of hernia should be employed in closing the wound; but if the patient is in a weakened state, so that haste is of all importance, this may be omitted and the wound closed by ordinary suture; or the canal may be packed with gauze and the patient put to bed.

If the intestine be gangrenous, there is nothing to do but a resection, either repairing the intestine at once or at a later period. To determine whether or not this should be done at once requires the exercise of much judgment, and there is no doubt that one must be largely influenced by the patient's general condition. Should it be decided that the patient is too weak to stand an immediate repair of the resected bowel, an artificial anus should be established by suturing both ends of the intestine to the wound, and the repair can be completed later. But if the patient's strength will warrant it the bowel should be closed at once by an end-to-end suture, either by means of the Murphy button or by the modified Maunsell method.

The subject of technique for these operations and for that for radical cure of hernia cannot be dwelt on in this paper.

In closing, I would like to emphasise certain points:

1. The symptoms of strangulated hernia are distinct, and diagnosis can always be made.
2. Strangulated hernia is a purely mechanical condition, and can only be relieved by mechanical means.
3. Taxis is dangerous, and should never be employed.
4. Strangulated hernia is a fatal disease if not promptly relieved.
5. Strangulated hernia should never prove fatal if properly treated at a sufficiently early time.

ON SUTURES.[1]

BY PROFESSOR W. PRITCHARD.

In sewing wounds many modes are had recourse to. I shall only deal with a few of them, the manner in which it is done, the results which follow, and whether the sutures should be left permanent or otherwise.

I have in my mind more particularly the wounds which we have to deal with in connection with horses, though of course we have to deal with wounds in all kinds of animals, or at all events of quadrupeds. There is one thing I would advise in connection with wounds in horses, and I want to lay particular stress upon it. However big the wound may be, if the parts are brought together fairly, the sutures should not be left in more than 24 or at the very outside 48 hours. I know this will meet with some contention, but I do not care about that. You will find from experience that if you bring the parts together for 24 or 48 hours, and then remove the stitches or sutures, although the wound may gape very considerably the parts will come together exactly where you stitched them in the first instance. I have in my mind two cases, one of the quarter of a horse where I should think three or four pounds of material was absolutely down away from its original position. The parts were brought together by common sutures, which were taken out and the wound healed perfectly well, and the parts came together exactly where they were stitched. Another case was in the fore arm of a horse, and I should think there was at least 2 lbs. of material hanging over the knee. I operated on that; I brought the parts together, and I took the stitches out in 24 hours afterwards. That wound healed, and the parts came together exactly where I

[1] Read before the Central Veterinary Medical Society.

had stitched them. You will ask what is the benefit of this. I will tell you. As you go about the country you will see horses that have had wounds stitched up, and you will see where the stitches have been, and a mark, generally about an inch in length, where those stitches have been put in, and instead of being taken out as I suggest and advise they have been allowed to slough out. This is one point in connection with sutures which I think should be thought of, and I am certain that it is advantageous after 24 or 48 hours to take the stitches out.

Just a word or two with regard to the character of the stitches. The horse's skin as a rule, taking the general surface of its body, heals by suppuration and not by first intention. But there are parts of his body where sutures are used or should be used, and healing does take place by the first intention. In those cases I object to using ordinary stitches. For example, take a torn eyelid. If you stitch that in the ordinary way with a thread and needle you will not get as good a union by first intention as if you have recourse to other means. And I think there is no suture you can use in connection with a torn eyelid which is half so serviceable as the pin and a little bit of tow or thread—I prefer tow—twisted round the edge of the pin. Here I should leave the suture for a little more than 48 hours, but I should have recourse to it in a way that would enable me absolutely to remove the stitch or suture without parting the edges of the skin. I should not use an ordinary pin, but there is a pin made of steel which is easy to insert, and having inserted it and twisted the tow round it, it is as easily removed. It is advantageous to use because it easily pierces the skin; it is just as easy to twist round the little tow that you use, and bring the surfaces together, and it is just as easily removeable, leaving the tow as a sort of bandage upon the part and keeping the part in connection.

Now I will tell you why that operation is successful as a rule, and why it is not successful in other parts of the body. In the eyelid you have a very vascular surface, the skin which covers it, and you have a very vascular surface on the internal surface, the mucous membrane, and on account of the vascularity of the two surfaces you get Nature's endeavours twice—on the inside and the outside, and healing by the first intention readily takes place. Then we have other parts of the body where the skin is thin and where you have a double layer of it. Take for example a torn nostril. You have got a layer of skin on the outer side, and of course I need not tell you that it is an extremely vascular structure, and you have a layer of skin on the inner side of it just as vascular as that upon the outer surface. Bringing those two parts together in the manner I have described, you get Nature's efforts on both sides, and as a rule healing by the first intention or by the adhesive process.

With regard to the kind of suture, we have thread, silk, wire, catgut, and a number of others. Perhaps I am an old-fashioned surgeon in this respect, but I do not mind that—I have never used any suture with a needle equal to thread. There is a good deal said about catgut sutures and their absorption, but I have never seen what I have read about, although I have tried them over and over again. I do not say it does not take place, but if I were stitching a big wound up to-night I should use good brown thread. I do not like wire. It very frequently breaks, and if it does not break there is a difficulty in taking it out. You might expect that I should say something about the wounds we make in operations, and I have in my mind just now neurotomy. I know some clever veterinary surgeons, in

performing the operation of neurotomy, put a stitch in the wound which they make in the leg to remove the nerve. You take my advice, gentlemen, when you perform neurotomy you won't make a wound sufficiently large in the skin to need a suture. And if you do, put the edges of the skin together with a pin, twist a little tow round it, and in 24 hours take it out again.

SHOULDER LAMENESS.[1]

BY W. HUNTING, ESQ., F.R.C.V.S.

I HAVE chosen this subject because it is wide enough to permit a good discussion, and because it is one which probably presents itself to town and country practitioners in not quite the same aspect. The work of horses on paved streets and their work in the hunting field or at plough are so different, that we may expect lameness which is common under one of the conditions to be uncommon under the other. In other words, some shoulder lamenesses will be more often seen in the country whilst others will be more frequently met with in town. Our united observation covers all conditions, and between us we ought to be able to clear up some doubtful points—perhaps add to the general knowledge.

The shoulder is an extensive region, and for surgical purposes must be considered to embrace all the structures of the fore limb from the withers to the elbow. The shoulders support the larger part of the weight of the body, and they play a part in progression not much inferior to that of the hind limbs, which are too often spoken of as solely concerned in propelling the body, while the fore are credited with mere weight sustaining.

The shoulder is anatomically made up of bones, muscles, joints, &c. like other parts of a limb, but it has the additional complexity of being attached to the trunk and neck by large muscles which extend to a distance from the shoulder proper. We must remember also the close relation of the shoulder to the walls of the thorax, and the possibility of its being to some extent affected by changes taking place within the chest. The difficulty of movement shown in cases of pleurisy, and the occasional cases in which some foreign body has passed from the stomach through the chest and impaled the shoulder, are illustrations of how the relative position of the shoulder to the thorax may lead to shoulder lameness.

The term shoulder lameness is used to denote a lameness which has its casual lesion in the shoulder, but I sometimes think it is used to imply a special and peculiar form of lameness which can be easily identified. If my surmise be correct, and the term is used to define a lameness which always has some diagnostic peculiarity, I venture to think it is wrongly used. In such an extensive region as the shoulder there are many different lesions leading to lameness. I know of no symptom common to all, and therefore hold that there is no distinctive and indicative symptom which is diagnostic of shoulder lameness in general. Diagnosis requires each case to be considered on its merits, and without any preconceived notion that a definite peculiarity either of position or motion must be apparent.

There are certain symptoms which observation has frequently con-

[1] Read before the Eastern Counties Veterinary Medical Association, February 22d, 1900.

nected with lesions of the shoulder, and very rightly so, but before accepting them as indicative we must be sure that they never arise from lesions in other parts of the limb. Let me just take some of these symptoms and discuss their value.

Circumduction of the leg.—This is a rather favourite symptom of shoulder lameness, and I acknowledge that it often is indicative—but not always. In some lamenesses having their seat in the knee, or even in the foot, I have seen circumduction as the most prominent peculiarity of movement. If a horse cannot raise a fore limb, or flex it so that the toe is removed from contact with the ground, he instinctively circumducts the foot to lessen the pain or difficulty of progression. Most lamenesses due to lesions in the shoulder interfere with the raising of the leg, and so we have the foot brought forward with an outward swing. But pain or fixation of the knee prevents the shortening of the limb, which is brought about by natural flexion, and so we have the toe liable to catch the ground. To avoid the pain or difficulty so caused, the horse in progression brings the foot forward by swinging the leg outwards. Circumduction is not then a movement peculiar to shoulder lameness.

Dragging the toe is a symptom of inability to raise the leg, and is perhaps a more diagnostic sign of shoulder lameness than any other. It may, however, result from injuries in the radial region.

Length of Step.—As far as I know, all shoulder lameness is accompanied by a shortening of the step—so is almost every other lameness of the fore limb.

Adduction and Abduction.—Carriage of a lame fore leg in a more outward direction than that of the usual line of motion is common enough in foot lamenesses. It is also seen in cases of injury to some of the pectoral muscles. Adduction is, I think, uncommon. I remember one case in which the foot when advanced was brought inwards. It rested in front of the other foot whilst the horse was standing still. Progression at a walk (trotting was impossible) displayed the most conspicuous adduction. A post-mortem examination disclosed fracture of the first dorsal vertebra.

Passive Motion.—Movement of a lame leg by human action may sometimes give a useful indication. Probably most practitioners, when a case of lameness presents itself with no definite symptoms, lift the foot and move the limb backwards and forwards, outwards and inwards. I do, but I confess with very rare success in eliciting anything diagnostic. The result, in fact, is often only to produce additional bewilderment. Quite sound horses object to over-extension of the shoulder and to over-flexion. Passive motion seems to me of little value except in determining whether or not knee, elbow, and shoulder allow of the usual amount of movement. We sometimes find that a joint cannot be properly moved to its normal limits.

Manipulation.—By this I mean, not the movement of parts, but the pressure exerted by hands or fingers. In relation to shoulder lesions I think this is the most deceptive and misleading form of examination. All horses object to pressure about the joints and muscles of the shoulder, and manipulation should never be relied on except after repeating the process on the sound side. Exceptionally misleading is the result of handling a part which has been blistered, or even irritated by liniments.

Position of limb at rest.—In some one or two shoulder lamenesses the position of the leg is very helpful, but in many it affords no indication of

the seat of the lesion. As a rule the fact of the foot on the lame side being in advance of or behind the level of the opposite foot is no sign of the seat of lameness. Even in such marked cases as those known as radial paralysis you may see the foot in both positions within a few minutes.

An old surgeon once described amaurosis as "a disease in which the patient sees nothing and the surgeon nothing." He was more witty than exact. There are shoulder lamenesses in which the patient feels something but the practitioner sees nothing. We may be firmly and rightly convinced that a horse is lame in the shoulder, but we are quite unable to point to any definite symptom. I am afraid that a strong conviction of this sort may not be free from error. Fortunately rheumatism does affect horses, and it may have its seat in the shoulder region. How very seldom can we locate the mischief, or give a logical and convincing reason for our belief to another practitioner. Action, manipulation, and passive motion all seem to lend themselves equally to support or deny a diagnosis. I must confess that I do not know how to diagnose shoulder rheumatism. I may suspect it, but I never feel certain.

Occasionally we come across a case of occult lameness in a fore leg which exists for a week or two and then is followed by the development of farcy in the limb. A post-mortem, it is said, has disclosed lesions in the lymphatic glands under the shoulder. I have not seen such a case, but I do not deny its existence. How would such a lameness be differentiated from rheumatism until too late to afford useful information? A merely negative diagnosis is always unsatisfactory, and yet it may be correct. When we fail to find any lesion in other parts of the limb, when the movement of the horse exhibits distinct difficulty in advancing the leg or in raising it from the ground, we may suspect shoulder lameness, but the diagnosis is uncertain.

With these introductory remarks I may pass on to some lamenesses which without doubt have their seat in the region of the shoulder. In each there is some symptom or group of symptoms differentiating the condition. Possibly more than I recognise may be known to some of the practitioners present. There may also be an explanation forthcoming of symptoms I have seen but am unable to explain. Various injuries may cause hidden lesions, accompanied by lameness. According to the structures affected we should expect to find some guidance from action or manipulation, and in some cases we may acknowledge that the lesion exists without any definite indication of its whereabouts. Twice in my life I have seen lameness of a very marked character arise from putting a horse in harness and causing it to draw a slight load. As soon as draught ceased lameness ceased, and the horse could be run in hand without a sign of pain or halting. Not a visible indication of anything wrong could be found on examination of neck or shoulder—no sign of enlargement or atrophy, no symptom of pain could be detected. I connect the lameness with the collar, but I fail to explain the why or wherefore.

Sprains of shoulder muscles probably occur, and are accompanied by pain and lameness. Such cases one would expect to show some definite local signs, such as swelling, pain on pressure, or alteration of bulk. I confess ignorance of any such cases. Sprain of muscle I think is uncommon except in one special form, and this brings me to the first of the well recognised shoulder lamenesses.

Shoulder-slip.—This is apparently a lameness due to sprain of the

spinati muscles. I say apparently, because it is possible to have lameness with atrophy of muscles which are not the seat of the primary lesion. It has not fallen to my lot to see a case of "shoulder-slip" in the first stage, and therefore I can say nothing of the symptoms. As this lameness is said to occur in young horses at plough, there are no doubt many of you who can supply information on this point. In the next stage—when acute symptoms have subsided, but atrophy of muscle exists—I have seen horses trotting sound. In cases where lameness persists, with some wasting of muscle, I cannot say I have always been able to detect any marked peculiarity of action. The marked abduction of the limb, and the outward rolling of the shoulder which sometimes accompanies wasting of the spinati muscles, may perhaps be evidence of a definite lesion. The sudden outward movement of the shoulder-joint occurs when the foot is on the ground, and at the time the greatest weight is placed upon it. This symptom is explained by the relaxation of the spinati tendons which form external ligaments of the shoulder-joint. These tendons may be relaxed as the result of paralysis of the spinati muscles, but are they relaxed by sprain of the muscles? Muscles may be paralysed as the result of injury to the nerve supplying them, and atrophy follows without any sprain.

As I have already said, I have no experience of the first stage of these cases. If the outward movement of the shoulder-joint is seen before any wasting of muscle is noticeable, or without any soreness or swelling of muscle, we might almost conclude that sprain of the muscles is not the cause.

This lameness is described in Dollar's translation of Möller's surgery as "paralysis of the supra-scapular nerve." This nerve supplies the spinati muscles, and therefore wasting may arise from the want of innervation. The outward slip of the shoulder is doubtless due to relaxation of the tendons, however brought about, and Möller states that "section of the tendon of the postea-spinatus results in precisely similar lameness." How does work in a plough bring about paralysis of the supra-scapular nerve? Is there not always an undetected injury? Could such injury result from the collar? It seems to me that the exact seat of injury and the sequence of results is not yet fully understood. The symptoms first shown by the horse need to be carefully observed and recorded. The verification of the diagnosis by a careful post-mortem examination would also be most acceptable.

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performing the operation of neurotomy, put a stitch in the wound which they make in the leg to remove the nerve. You take my advice, gentlemen, when you perform neurotomy you won't make a wound sufficiently large in the skin to need a suture. And if you do, put the edges of the skin together with a pin, twist a little tow round it, and in 24 hours take it out again.

SHOULDER LAMENESS.[1]

BY W. HUNTING, ESQ., F.R.C.V.S.

I HAVE chosen this subject because it is wide enough to permit a good discussion, and because it is one which probably presents itself to town and country practitioners in not quite the same aspect. The work of horses on paved streets and their work in the hunting field or at plough are so different, that we may expect lameness which is common under one of the conditions to be uncommon under the other. In other words, some shoulder lamenesses will be more often seen in the country whilst others will be more frequently met with in town. Our united observation covers all conditions, and between us we ought to be able to clear up some doubtful points—perhaps add to the general knowledge.

The shoulder is an extensive region, and for surgical purposes must be considered to embrace all the structures of the fore limb from the withers to the elbow. The shoulders support the larger part of the weight of the body, and they play a part in progression not much inferior to that of the hind limbs, which are too often spoken of as solely concerned in propelling the body, while the fore are credited with mere weight sustaining.

The shoulder is anatomically made up of bones, muscles, joints, &c. like other parts of a limb, but it has the additional complexity of being attached to the trunk and neck by large muscles which extend to a distance from the shoulder proper. We must remember also the close relation of the shoulder to the walls of the thorax, and the possibility of its being to some extent affected by changes taking place within the chest. The difficulty of movement shown in cases of pleurisy, and the occasional cases in which some foreign body has passed from the stomach through the chest and impaled the shoulder, are illustrations of how the relative position of the shoulder to the thorax may lead to shoulder lameness.

The term shoulder lameness is used to denote a lameness which has its casual lesion in the shoulder, but I sometimes think it is used to imply a special and peculiar form of lameness which can be easily identified. If my surmise be correct, and the term is used to define a lameness which always has some diagnostic peculiarity, I venture to think it is wrongly used. In such an extensive region as the shoulder there are many different lesions leading to lameness. I know of no symptom common to all, and therefore hold that there is no distinctive and indicative symptom which is diagnostic of shoulder lameness in general. Diagnosis requires each case to be considered on its merits, and without any preconceived notion that a definite peculiarity either of position or motion must be apparent.

There are certain symptoms which observation has frequently con-

[1] Read before the Eastern Counties Veterinary Medical Association, February 22d, 1900.

nected with lesions of the shoulder, and very rightly so, but before accepting them as indicative we must be sure that they never arise from lesions in other parts of the limb. Let me just take some of these symptoms and discuss their value.

Circumduction of the leg.—This is a rather favourite symptom of shoulder lameness, and I acknowledge that it often is indicative—but not always. In some lamenesses having their seat in the knee, or even in the foot, I have seen circumduction as the most prominent peculiarity of movement. If a horse cannot raise a fore limb, or flex it so that the toe is removed from contact with the ground, he instinctively circumducts the foot to lessen the pain or difficulty of progression. Most lamenesses due to lesions in the shoulder interfere with the raising of the leg, and so we have the foot brought forward with an outward swing. But pain or fixation of the knee prevents the shortening of the limb, which is brought about by natural flexion, and so we have the toe liable to catch the ground. To avoid the pain or difficulty so caused, the horse in progression brings the foot forward by swinging the leg outwards. Circumduction is not then a movement peculiar to shoulder lameness.

Dragging the toe is a symptom of inability to raise the leg, and is perhaps a more diagnostic sign of shoulder lameness than any other. It may, however, result from injuries in the radial region.

Length of Step.—As far as I know, all shoulder lameness is accompanied by a shortening of the step—so is almost every other lameness of the fore limb.

Adduction and Abduction.—Carriage of a lame fore leg in a more outward direction than that of the usual line of motion is common enough in foot lamenesses. It is also seen in cases of injury to some of the pectoral muscles. Adduction is, I think, uncommon. I remember one case in which the foot when advanced was brought inwards. It rested in front of the other foot whilst the horse was standing still. Progression at a walk (trotting was impossible) displayed the most conspicuous adduction. A post-mortem examination disclosed fracture of the first dorsal vertebra.

Passive Motion.—Movement of a lame leg by human action may sometimes give a useful indication. Probably most practitioners, when a case of lameness presents itself with no definite symptoms, lift the foot and move the limb backwards and forwards, outwards and inwards. I do, but I confess with very rare success in eliciting anything diagnostic. The result, in fact, is often only to produce additional bewilderment. Quite sound horses object to over-extension of the shoulder and to over-flexion. Passive motion seems to me of little value except in determining whether or not knee, elbow, and shoulder allow of the usual amount of movement. We sometimes find that a joint cannot be properly moved to its normal limits.

Manipulation.—By this I mean, not the movement of parts, but the pressure exerted by hands or fingers. In relation to shoulder lesions I think this is the most deceptive and misleading form of examination. All horses object to pressure about the joints and muscles of the shoulder, and manipulation should never be relied on except after repeating the process on the sound side. Exceptionally misleading is the result of handling a part which has been blistered, or even irritated by liniments.

Position of limb at rest.—In some one or two shoulder lamenesses the position of the leg is very helpful, but in many it affords no indication of

the seat of the lesion. As a rule the fact of the foot on the lame side being in advance of or behind the level of the opposite foot is no sign of the seat of lameness. Even in such marked cases as those known as radial paralysis you may see the foot in both positions within a few minutes.

An old surgeon once described amaurosis as "a disease in which the patient sees nothing and the surgeon nothing." He was more witty than exact. There are shoulder lamenesses in which the patient feels something but the practitioner sees nothing. We may be firmly and rightly convinced that a horse is lame in the shoulder, but we are quite unable to point to any definite symptom. I am afraid that a strong conviction of this sort may not be free from error. Fortunately rheumatism does affect horses, and it may have its seat in the shoulder region. How very seldom can we locate the mischief, or give a logical and convincing reason for our belief to another practitioner. Action, manipulation, and passive motion all seem to lend themselves equally to support or deny a diagnosis. I must confess that I do not know how to diagnose shoulder rheumatism. I may suspect it, but I never feel certain.

Occasionally we come across a case of occult lameness in a fore leg which exists for a week or two and then is followed by the development of farcy in the limb. A post-mortem, it is said, has disclosed lesions in the lymphatic glands under the shoulder. I have not seen such a case, but I do not deny its existence. How would such a lameness be differentiated from rheumatism until too late to afford useful information? A merely negative diagnosis is always unsatisfactory, and yet it may be correct. When we fail to find any lesion in other parts of the limb, when the movement of the horse exhibits distinct difficulty in advancing the leg or in raising it from the ground, we may suspect shoulder lameness, but the diagnosis is uncertain.

With these introductory remarks I may pass on to some lamenesses which without doubt have their seat in the region of the shoulder. In each there is some symptom or group of symptoms differentiating the condition. Possibly more than I recognise may be known to some of the practitioners present. There may also be an explanation forthcoming of symptoms I have seen but am unable to explain. Various injuries may cause hidden lesions, accompanied by lameness. According to the structures affected we should expect to find some guidance from action or manipulation, and in some cases we may acknowledge that the lesion exists without any definite indication of its whereabouts. Twice in my life I have seen lameness of a very marked character arise from putting a horse in harness and causing it to draw a slight load. As soon as draught ceased lameness ceased, and the horse could be run in hand without a sign of pain or halting. Not a visible indication of anything wrong could be found on examination of neck or shoulder—no sign of enlargement or atrophy, no symptom of pain could be detected. I connect the lameness with the collar, but I fail to explain the why or wherefore.

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rib. This lesion is directly under the brachial plexus of nerves, and therefore there is no difficulty in accounting for all the symptoms.

Fracture of the first rib is not the only cause of "dropped elbow," but the most common cause. I have had the opportunity of making two post-mortem examinations of horses which suffered from this form of lameness, recovered, and worked for many months after. In one I found complete union of the fractured bone, in the other a fibrous union with some displacement.

As a rule weeks or even months are required to allow of such a recovery as will permit the resumption of work; this we should expect from the nature of the lesion. But cases have been seen by practitioners where recovery took place in a few days. These can hardly be credited to fracture of the first rib, and we are left to decide whether some direct injury to the nerve or muscle may not occasionally cause the characteristic symptom of dropping of the elbow. Fractures differ widely in degree, and the specimens I produce will show, to some extent, how the lesion might cause a very short or a long paralysis of the muscles. I do not deny the production of radial paralysis by other lesions than fracture of the first rib, but I am convinced that this fracture is by far the most common cause.

It is worth noting that sometimes both first ribs are fractured.

Arthritis and Bursitis.—As the result of injury to the point of the shoulder, we have inflammatory changes set up, sometimes in the shoulder-joint, sometimes in the bursa of the flexor-brachii tendon, and occasionally in both. I am unable to differentiate between an inflammation of the joint and one of the bursa. In practice the value of a diagnosis would be that we might safely advise slaughter at once if the joint were affected, whereas we might try the effect of treatment on the cases of bursitis. Even in the cases of inflamed bursa, my experience would warrant the very worst prognosis being made in most cases—in all that cannot bear weight on the limb after the lapse of a period of eight or ten weeks. The changes resulting in the affected structures from bursitis are very marked. The cartilages become destroyed and the tendon ossified. In some cases the ossification extends down into the flexor-brachii muscle. Does this ossification of the muscle ever arise from sprain? I am inclined to doubt it, as in every case I have seen the commencement of lameness was traceable to injury of the point of the shoulder.

Dislocation of the shoulder-joint I have never seen in the horse. The recorded cases are not altogether convincing, and I should like to hear of a well authenticated example of the injury. The anatomical conditions of the joint would suggest that dislocation is impossible without fracture, but it would be rash to deny the possibility.

Fractures as a cause of shoulder lameness are uncommon. In addition to those of the first rib we may have either scapula or humerus broken. Fracture of the humerus is usually easy of diagnosis from the displacement of the parts. Fracture of the scapula may be difficult to detect when there is little or no displacement. The question of displacement seems to be the best guide to prognosis. If we cannot detect displacement there is a probability of union and recovery. If we have displacement the chances of union are so slight that we may safely suggest slaughter.

Compound fractures of the scapula, even when only a small piece of bone is detached, are grave cases, as the violence which inflicts them damages the soft tissues to a degree rendering repair without much swelling and suppuration unlikely.

RABIES.[1]

Some Early Observers.

Van Swieten in 1771 gave a very good description of the disease, and this is of special interest in connection with some of the controversies that have occurred in the last ten years, more especially with reference to the work of Pasteur. It is of interest to remember that Van Swieten recognised that there were two forms of the disease—a furious form and a paralytic form. So that more than a hundred years ago the paralytic form of rabies was recognised and described. Of course, it was not then described as fully as we know it now, but it was definitely recognised, and that is a point of very great interest when we remember that a few years ago—I think in 1885—when a well-known case of paralytic rabies occurred in the human subject it was asserted by many persons, well-intentioned, no doubt, that it was a new disease which had been produced by Pasteur's methods, and that it was not a disease that was known to the older writers. A French observer, Gohier, made a considerable number of observations on rabies, and he was one of the earliest, if not the earliest, to experiment on rabies in the laboratory. Many of his observations have stood the test of time, but one of his facts—or rather one of his supposed facts—I think has not stood that test. He thought that the carcase of the animal was highly infective and that the disease was spread in that manner. Subsequently to 1811 we come to Grüner, who in 1813 made important observations, and I believe he was the first to determine the fact that the saliva was infective. That, perhaps, is the most important observation that was made from the time of Van Swieten. Van Swieten's observations, recognising that the disease existed in two forms, was a very important one, and Grüner's observation that the saliva was infective was similarly a most important one. The next observer who worked extensively at the subject was Berndt, whose work was published in 1822, and who confirmed the observations of others, and who corroborated the original observations of Van Swieten that the disease existed in two forms—a furious form and a tranquil form. I believe he was the first to use those terms, the tranquil form now being usually spoken of as "dumb rabies." That brings us to the year 1822. The next most important observation was that of Galtier in 1871, who described the disease in rabbits. Inasmuch as the disease in rabbits is totally different in its main symptoms to that in dogs, the recognition of it in the rabbit and the description of the typical symptoms in that animal was a very distinct advance. In recent times the most important observations up to the time of Pasteur were those of Nocard and Paul Bert in 1880. These observers showed that if the saliva was filtered through plaster the virus remained on the plaster; in other words, they showed that the poison of rabies—the exact nature of which we do not know even at the present time—was a particulate virus.

Pasteur's Work.

Then we come to the work of Pasteur. In five years, and that was about the time that Pasteur's work on rabies extended over—in speaking of Pasteur I, of course, mean the work of Pasteur and his pupils—the

[1] Abstract of two Lectures delivered by Professor Rose Bradford.

additions to our knowledge of rabies were far greater than those of all other observers put together. That affords an illustration of what I said a few minutes ago, that rabies is perhaps the most striking instance of a few years' experimental work in the laboratory adding more to our knowledge than, I was going to say, centuries of accurate and careful observation. Pasteur's most striking and important discoveries in connection with rabies may be said to be four in number. He demonstrated first of all that the virus was not only in the saliva but that it was also in the nervous system. That was the first fact which Pasteur and his pupils showed, and they proved that not only was the virus present in the central nervous system, but that it was present also in the peripheral nervous system. It had been suggested—I do not know when, but some considerable time ago—that this was so in the case of tetanus, and long before the bacterial origin of tetanus was known it was surmised that the poison of tetanus travelled by the nerves. In the older surgical writings observations are recorded with reference to the part played by the irritation of small nerves in wounds as a cause of tetanus. It is in rabies, and as a result of Pasteur's work, that we have our best instance of the poison of a fatal disease producing its effects by acting on the nervous system, and not only producing its effects by this means, but apparently reaching the central nervous system by travelling along the peripheral nervous system. The foundation for that view of the pathology of rabies was laid by Pasteur's discovery that the poison was found in the central nervous system and also in the peripheral nervous system.

Pasteur's second discovery was the ease with which the disease could be directly inoculated, provided that it was inoculated in the nervous system. A great many observers had previously tried to inoculate rabies. Some had succeeded and some had failed, but the fact that they used the subcutaneous method of inoculation or the method of simply rubbing the virus on a raw surface in the skin is the probable explanation of the very great difference in the recorded facts. Some observers had even gone so far as to say that the saliva was not infective; others found that the saliva was infective; others found certain organs to be infective and not other organs. There were great discrepancies, largely dependent on the fact that the method of inoculation, as we shall see later, was not quite a satisfactory one. Pasteur added very greatly to our knowledge by showing that the inoculation into the nervous system was the best way of communicating rabies for experimental purposes from one animal to another; and the method which Pasteur invented and elaborated with reference to rabies has, of course, since his time been used, not only with so much success in the case of rabies, but with certain success in the study of the pathology of other diseases. Both those facts Pasteur observed in 1881. Thirdly, in 1884 he made his first attempts, or at any rate he got his first results in his attempts, at attenuating the virus. He proceeded first of all on the lines he had pursued in studying similar problems in other diseases,—viz., by seeing the effects of inoculating a series of animals of different species. After trying a large number of animals Pasteur found that if a succession of monkeys were inoculated the virus became attenuated, and finally, after passing through a considerable number of monkeys, that the poison did not produce such marked effect on dogs as it had originally produced. Conversely he found that by passing the virus through a series of rabbits it was increased in potency. But he very

soon came to the conclusion that he could not hope to get curative effects or to elaborate any means of treating the disease by the results obtained in monkeys. Lastly, in 1885, he elaborated the very remarkable method which goes by his name for treating the malady, or more especially for preventing the malady in animals and in man. I do not propose to discuss that at any length; but you will remember that it consists in getting a virus of a certain fixed strength which he called *le virus fixe*, and this he obtained by passing the disease through a succession of rabbits—I think about 100—until the period of duration of the disease and the period of incubation of the disease are both absolutely fixed. It is well known that if you take the sporadic cases of rabies which occur in the streets sometimes the virus from one dog will produce a malady with a short incubation period, and the disease will run a rapid course, while in another case it will have a long incubation period and may run a very much slower course. Pasteur thought that it was essential to work with a virus of constant strength, and he obtained this virus of constant strength by passing it through a succession of rabbits. Having done that, he found that the virus was attenuated by drying, so that if the spinal cord of the rabbit be removed and dried he found that in the course of a fortnight the virus had completely lost its strength. Those who have seen Pasteur's Institute may remember the darkened room, which is kept at an equable temperature, in which these spinal cords of the rabbits are dried. Therefore, by preparing separate extracts of these spinal cords every day, extending over a period of a fortnight, a virus of different degrees of potency could be obtained, and that forms the basis of his method of treating the disease, which method I do not propose to discuss now.

Since 1885 our knowledge of the nature of rabies has not really greatly progressed. A number of observers have recorded that they have isolated an organism in cases of rabies, but at the present time there is no consensus of opinion amongst bacteriologists with reference to this question. I think it cannot be said that the identity of the organisms of rabies has been clearly established, although, of course, there are many strong arguments in favour of the disease being of microbic origin, quite apart from the observation of Nocard and Paul Bert which I have mentioned, that filtration of the saliva through plaster removed the virus.

That is a very short historical sketch of the progress of a portion of our knowledge with reference to the nature of rabies. There is one other point which one would like to go into before dealing with the immediate question of diagnosis, because one cannot really discuss the diagnosis without first discussing this. I refer to the question of the animals that are liable to be affected by the disease.

Liability to Infection.

Rabies, as you know, is a disease which is common to man and to animals, but all animals are not affected with equal ease nor does the disease run the same course in all animals. It affects all the mammalia, and it also affects birds, but it is very doubtful whether it affects the amphibia. It is said by one observer to do so, but others have been unable to confirm his observations. So we may say that the disease is limited to the mammalia and to birds. As a matter of fact, in the case of birds, it is mainly seen as the result of experimentation. It is very difficult,

as would occur to any one, for a bird to be inoculated from a bite for obvious reasons; amongst other reasons is the fact that the feathers would probably remove the virus, so that the question of the disease affecting birds is not one of any great practical importance, although there is a considerable degree of theoretical interest about it, because birds are more refractory to rabies than are mammals. In mammals, as you know, the carnivora are those which are especially affected, and you know also that it is a disease which affects not only domestic animals but also wild animals. Some of the most serious outbreaks of rabies, both in animals and in the human subject, have occurred owing to its prevalence in wild animals. As regards wild animals, those of most importance from our point of view are wolves and foxes. Wolves, of course, have been a frequent source of rabies, both in France and in Russia, more especially in Russia, and it is not uncommon, I think, for outbreaks of the disease in this country to be associated with its prevalence in foxes. As regards other animals, the disease is not uncommon in cattle, and it is more common in horned cattle. It is more common in cattle than in goats. It is not very rare in horses, but it is much rarer in horses than in cattle. It is exceedingly rare in pigs. A suggested and a probable explanation of this fact is that there is a very large amount of subcutaneous fatty tissue in the pig, and, as we shall see later, subcutaneous fat is not at all suitable for the inoculation of rabies. Therefore, the disease is common in dogs and cats, not uncommon in wolves and foxes, but in other animals it is rare—most rare in the pig, less rare in the horse, and least rare in cattle.

Prevalence.

Another interesting point in reference to rabies is that, like many other diseases, perhaps nearly all diseases, it exists in certain localities and is very difficult to eradicate from those localities. Then at intervals of longer or shorter duration there are epidemics of rabies. In this country, for instance, Lancashire has long had an evil repute as a district where rabies is more or less endemic, and the same is true of Ireland, and of certain districts in Germany and France. As regards epidemics, there was a very severe epidemic indeed in France and in Germany, and, in fact, in the whole of the western side of Europe, in the years from 1720 to 1723. In this country the best known epidemics of rabies—of course, I am speaking of rabies in animals—were from 1754 to 1762. There was a very severe epidemic in Lancashire in 1864. Then, more recently, there was what I think I may call a severe epidemic in the years 1894-96, and in 1895 and 1896 there was a very considerable amount of rabies in London. In 1897 there was not so much, in 1898 still less, and this last year I think I am correct in saying that there were no cases of rabies, anyhow none came under my observation. There have been epidemics amongst dogs and cats, but in addition to that in this country there have been severe epidemics amongst deer. The most interesting and extensive epidemics amongst deer have been these. In 1795 there was an attack of rabies amongst the deer in the Royal Parks, then there was a severe epidemic in 1872 in herds of deer belonging to the Duke of Westminster, and it will be within the recollection of most of you that there was a very severe epidemic in 1886 among the deer in Richmond Park, where amongst 1200 animals 264 were affected with rabies. These are the principal epidemics of the disease which have occurred in this country in recent times.

There are certain countries in which rabies is unknown, and amongst these countries are Australia and Norway. There are other countries where outbreaks of rabies have occurred as the result of the importation of dogs, and, it is said, more especially of sporting dogs. There is an impression that is very current that rabies is a malady which is especially apt to occur amongst uncared-for dogs, whereas that is not at all the true state of affairs. Rabies is a disease which may occur in any dog, and we are always struck with the considerable number of cases in what are commonly called sporting dogs which are sent to us for diagnosis. There are three epidemics which have been laid to the charge of sporting dogs taken from this country. One of these was in the Mauritius in 1813, one was in Malta in 1847, and one was at Shanghai in 1867.

Method of Communication.

How is the malady communicated? In the great bulk of cases it is communicated by bites, and that is one of the very reasons why the prevalence of it amongst the carnivora is so important for us to recognise. When it is communicated by a bite you know that, on the whole, the cat is the more dangerous animal, owing to the fact that the cat is liable to "fly," as it is said, to the more exposed part of the body—the face, for instance—and also, perhaps, owing to the fact that it is well known that bites in the face, for reasons we will consider later, are more dangerous than bites in other parts of the body. Rabies, perhaps, can also be communicated by licking—that is to say, by licking a raw surface on the body. But it must be always remembered that the virus of rabies is not one that is very easily inoculated, and that it is much more difficult in the laboratory to communicate rabies by smearing a raw surface with the saliva than it is by injecting it hypodermically. It is more difficult to communicate it hypodermically than by injection into the muscles, and it is more difficult to communicate it by injecting it into the muscles than by injecting it into the eye. Further, it is more difficult to communicate it by injection into the eye than by introducing it subdurally. So that I do not know that it can be said that it has been accurately proved that it can be communicated by licking, but at any rate it is probable. It is said that when people are bitten by a rabid animal from 10 to 20 per cent. of the sufferers succumb to rabies. That is a matter on which it is most difficult to get statistics. Any one who has had occasion to look the matter up will find very varying statements with reference to this point. Apparently, however, the balance of evidence is in favour of that view. A French observer, Leblanc, who collected a great number of accurate statistics—accurate, that is, as far as one can see—stated that the percentage was about 16. There is a very celebrated case quoted by Hunter where one mad dog bit 21 people and only one person succumbed to the disease. That has been sometimes quoted as showing that the mortality of the disease is only 5 per cent., but of course that is erroneous. The disease can also be communicated by infection at post-mortem examinations; and there have been instances of that among veterinarians. I am not aware of any recorded instance of such infection by the human subject—I mean of a human being having been infected by doing a post-mortem examination on a case of rabies in man—but there are cases on record in which it has occurred to veterinary surgeons. The third way in which it is said to be communicated is by the ingestion by animals of the flesh of other animals which have died from the

disease. Here, again, there has been great controversy, many observers having failed to communicate the disease in that way and others having succeeded. Of course, an obvious explanation occurs—viz., that the question as to whether an animal contracts rabies or not by eating the flesh of an animal dying from rabies is whether there is any lesion in the throat or alimentary canal or not. Fourthly, it has been shown that it can be communicated by the milk of a suckling animal or of a suckling human being. There is a case recorded in the West Indies where an infant was suckled by its mother who was suffering from the early stages of rabies, and the child contracted the disease and died from it before the mother. It has been proved in the laboratory that the milk of suckling animals, and human milk in case of rabies in women, is infective. Still, that is a point of only scientific interest.

Lastly, it has been shown in the laboratory that it can be communicated through the mucous membranes. The French observer, Galtier, has shown that if the virus of rabies be put into the conjunctival sac or into the interior of the nose a certain proportion of the animals so treated will succumb to rabies. These are really only matters of scientific interest. For all practical purposes the disease is communicated by biting, and you know that the danger of a bite depends very largely upon the part of the body upon which it is situated, quite apart from the question of clothes. Of course, it is obvious that bites in the legs and in the feet are less dangerous than bites in the hands or face, owing to the probability of the saliva being wiped of by the clothes or the boots. But, apart from that, there is, apparently, evidence that the inoculation of the virus into the face, for instance, is more liable to be followed by a rapid onset of the disease than if it is inoculated in the legs. The well-known experiment of Roux in which an animal was taken, the spinal cord was divided, and then the virus of rabies was inoculated into the hinder portion of the animal, throws some light on this question. Under those circumstances it can be shown that the lower segment of the spinal cord, separated from the central nervous system, will contain the virus in great quantity and in a high degree of virulence at the time when the upper segment does not contain it. And so it has been thought that the proximity of a part to the central nervous system does play a part and is a point of importance both in the progress and in the rate of onset of the malady; so that a wound of the face, inasmuch as it is near the central nervous system, is for that reason more dangerous. There are one or two other general points which I may mention.

Nature of the Virus.

First of all, let me say a few words with reference to the virus. I have already said—perhaps more than once—that rabies is a remarkable disease; but there is nothing, perhaps, in which it is more remarkable than in the nature and in the situation of the virus. We have seen that the virus is present in the saliva. We have also seen that it is present in the central nervous system and in the peripheral nervous system, but that it is not present in the blood. All observers are agreed that the blood of animals suffering from rabies is not infective. It is apparently not present in the bulk of the tissues and organs of the animal. It is not present in the muscles. It is not present in the liver or the spleen. It is present in the glands, such as the salivary glands, the pancreas, the lachrymal gland, the suprarenals, and the mammary gland; and it has always seemed to me,

since I have been working at the subject, that that is one of the most striking phenomena in pathology—that this potent virus should be present in the central nervous system and in the secreting glands, but should be absent from the blood and from the tissues such as those of the liver and the spleen. It is present, as I mentioned a minute ago, in the milk of suckling women; it is not present in the urine. It is said to be present in the seminal fluid, but in this there have been discrepancies among different observers. However, there have been no discrepancies as regards the saliva and those glands I have mentioned, nor as regards the milk. Then, as I mentioned a few minutes ago, the virus is slowly destroyed by being dried in the air; and it takes about a fortnight for it to lose its virulence under those circumstances. When exposed to the air and to sunlight it loses its virulence in about 14 hours. In water it will maintain its virulence for as long as 40 days—for from 20 to 40 days—and in glycerine (which is a point of some importance with reference to the technique of inoculation) it will maintain its virulence for as long as a month. Then an important point where one has to investigate suspected cases of rabies is the influence of putrefaction on it. It has been found by French observers that the virus remains potent in the carcase of a dog which has been buried for 44 days: it has been found potent in that of a rabbit 27 days after burial, and in that of a sheep 21 days after. So we have periods ranging from three to six weeks. My own experience does not quite agree with this. I know that I have failed twice to detect the presence of rabies in an animal which undoubtedly died from the disease where the carcase of the dog had been buried only a few days and had then been exhumed and sent to me. In one of those cases—a case in which a man had been bitten by the dog in the ear and had died from hydrophobia, so that one had very clear evidence quite apart from the suggested diagnosis of rabies from the symptoms in the dog—inoculations from that dog were quite ineffective. That was probably owing to the presence of putrefaction. It occurred in the hot summer months. But the French observers—and that is the only discrepancy between their results and such results as I have obtained—have always laid stress on the point that ordinary putrefaction does not prevent the recognition of the presence of rabies in a buried carcase. Of course, you may often fail to detect the presence of rabies in suspected cases if there happen to be other organisms present, especially organisms capable of causing septicæmia owing to the obvious fact that septicæmia runs a more acute course than rabies, and so perhaps both diseases may be communicated, but the animal dies from septicæmia before the symptoms of rabies have become manifest.

(*To be continued.*)

THE METHOD OF INFECTION IN BLACK-QUARTER.[1]

BY PROFESSOR STEWART STOCKMAN.

In animals the bacilli of black-quarter are contained mainly in the affected muscles and œdema. A few are present in the blood, but they are not numerous, and are only especially found about the time of death. The bacilli in the blood are too few to be detected by the microscope, but if

[1] Abstract of a Lecture delivered before the Western Counties Veterinary Medical Association.

some of the blood is drawn into a sterile tube, sealed up and incubated for twelve hours, bubbles will be produced, and cover-glass preparations and cultures can be made. Blood then, although it contains only a few bacilli, must be considered as virulent. The blood of an affected animal can have very little to do with the spread of the disease. It has something to do with it, of course, but not to any considerable extent, unless the carcase was opened and the microbe was freed. The organism is present in the soil in many districts, and remains there for a considerable time. It is saprophytic, *i.e.*, a facultative parasite. Black-quarter has disappeared from certain districts, probably because of improved drainage and treatment of the soil. Heavy showers of rain carry the microbes away with them; if there is no free drainage the organisms remain about the surface. Infection may take place by scratches, but there are few positive results. When the virulent material is injected into the subcutaneous tissues, or into the muscles, there is greater certainty of getting black-quarter, but not absolute certainty. Infection also depends upon where inoculation is performed. If the virus is injected into a place like the extremity of the tail, there will be a small percentage of positive results. If large doses of the poison are used in this position more positive results may be obtained. Experimental evidence teaches that it is difficult to infect an animal by way of the alimentary tract. A large amount of virulent material has to be used, and it is very probable that infection only takes place through the medium of wounds on some part of the tract. That, of course, was a subject about which one could not give a definite answer; but it is most likely to be the case as regards the respiratory tract. It is known that if the virus be injected into the trachea—without letting any get into the subcutaneous tissues—the animal does not take black-quarter. It shows a poison fever, but after that it is immune. When injected into the vein, black-quarter might be produced if large doses are used, but in most cases they would find the poison fever, and after that the animal was immune. If when the material is circulating in the blood a bruise is inflicted so as to let the microbes out into the muscles, black-quarter is likely to result. That is found after attempting to give animals immunity by injecting virus directly into the vein. It is found that a fewer number take black-quarter, and it is supposed to be due to something which allows the microbe to pass out. That could be done by inflicting a bruise. It may be asked why these experimental methods sometimes fail? The present paper deals mainly with this question. At present attention will be confined to trying to discover which of three paths of entrance is followed under natural conditions. Can we learn this from experiments?

Only two are worth discussing, viz., wounds or scratches, and the alimentary tract. Infection could be produced through the medium of wounds, and the further the virus is away from the oxygen the more likely are we to get positive results. Those wounds that came into contact with the soil might be regarded as the most dangerous. The microbes are not altogether on the surface of the soil, but as pastures become bare, or dry earth gets scraped away, microbes will be found underneath. Tumours seldom develop in the lower region of the limbs where the connective tissue is dense.

It might be supposed that this is the commonest place of infection. There is nothing againt the supposition that inoculation does not take

place there and pass by the blood-stream to the tissues of another part. Once in the circulation, something may occur to let the microbes out of the vessels at the shoulder or hip. The fact that tumours do not appear in the lower regions is not a strong argument that the infection does not take place through wounds in the lower parts of the limbs.

It is difficult to infect animals experimentally by way of the alimentary tract. In order to perform these experiments successfully, larger doses must be used than any animal would require under natural conditions. Alimentary infection may therefore be ruled out. It is possible, but it must be admitted that most evidence is in favour of wound infection.

All veterinarians who have come into contact with this disease must have puzzled over certain characters of the disease. Why, for instance, do calves and beasts over two and a half years possess a degree of immunity? The immunity is not absolute, certainly, but it exists. Why does the disease disappear for years from a pasture, and sometimes reappear after a year or two? Thomas, whose experience of this affection entitles him to receive the highest consideration, asserts that he has never seen black-quarter attack adult animals raised in an infected district, although those imported from non-infected districts easily contract the disease. Arloing, Cornevin, and Thomas think that most young animals living in infected districts become inoculated at one period of their lives with the black-leg virus. When the dose is small the animals show only a passing indisposition, which may be overlooked. They are afterwards endowed with a certain degree of immunity. If the dose is large they contract black-leg in a fatal form. Moreover, these authorities suggest that animals living on infected pastures may be subjected to repeated inoculations, which further increase the degree of immunity derived from the first infection.

There are serious objections to accepting these observations without reserve. If they are correct one would surely expect to find almost every animal to be immune if it survived the age of eighteen months or less in a black-quarter district. This, of course, is not the case. Any one who has experimented with black-quarter virus knows how irregularly even susceptible animals act towards it. Some calves, for instance, will develop the disease after a certain virus has been inoculated, while others resist. Another virus may fail to kill the majority of animals to which it is inoculated, and *vice versa*. Observations of this kind apply to the fresh as well as the dried virus. These are observed facts, and one usually seeks to explain them by suggesting a variation in the number or virulence of the microbes, or by an increased susceptibility of the animal.

These explanations are not altogether satisfactory. There is something wanting.

If a number of pathogenic organisms gain entrance to the tissues of a susceptible animal even in small numbers, they multiply sufficiently to produce the disease, unless they are overcome by the resisting forces of the body, *i.e.*, the phagocytes in particular. It is quite logical to suppose that a few microbes may be more easily overcome than a large number, and that the phagocytes will have more difficulty in destroying virulent forms than weaker ones. Under natural conditions of infection, however, only a very small number of microbes can gain entrance to the tissues in each case. Moreover, in the majority of cases it is not the rod forms but the spores which penetrate. It is doubtful if the spores often undergo a material diminution of virulence, for they are extremely resistant to what are

generally called adverse circumstances. They are not destroyed by a dry temperature of 100° C., maintained for six hours. I doubt if the spores are ever materially diminished in virulence. It has been my experience that the virus, even the so-called attenuated virus, never, or but very rarely, fails to give an unprotected animal black-quarter, if the material be mixed with a few drops of acetic acid.

Fresh or dried virus which has been attenuated may fail to kill unless it be mixed with acetic acid or some other material. Acetic acid, lactate of potash, salt, and other substances have been shown by Nocard and Roux to have this assisting action. Arloing previously demonstrated that ½ per cent. of a 5 per cent. solution of lactic acid increases the virulence of black-quarter poison. Nocard and Roux (*Annals of the Pasteur Institute*, 1887, p. 257) have demonstrated by experiment that these substances have the same predisposing effect even when injected at a part of the body different to that at which the virus has been introduced. I have found that a 5 per cent. solution of sodium carbonate has the same effect even when sterilised. The action, therefore, cannot be attributed solely to either the acidifying or alkalinising effect of the material injected. I may say, too, that bruising has the same effect. If the tissues are bruised at the seat of inoculation a positive effect is more likely. This is also the case if a large amount of toxin is injected along with the microbes. It has a predisposing influence. The toxin is obtained by filtering the muscle juices or from an artificial culture. These materials seem to act by enfeebling the phagocytic properties of the leucocytes. If certain other microbes, harmless in themselves, are mixed with the virus its virulence is increased, and black-quarter surely follows inoculation.

It is known, however, that the black-quarter bacillus is not always present in pure culture in the lesions in animals dead of the disease. There are often other bacteria present as well. The presence or absence of these auxiliary microbes explains in great part the variations in virulence of this or that toxin. When virulent muscle is heated to a temperature between 80 and 90 degrees C. its virulence apparently becomes attenuated. This temperature kills the auxiliary microbe and the black-quarter rods, but there is no material action on the spores of the latter, for the material regains its virulence when acetic acid is added. If other materials be mixed with the heated material its virulence is similarly restored.

In 1897 I performed some experiments on guinea-pigs to test the protective power of the muscle juice from black-quarter lesions, which had been deprived of microbes by filtration through porcelain. I thought it would probably give animals immunity. The results were not very encouraging, and they have not been published. They are interesting, however, as they throw some light on the question now under discussion. In order to test the degree of immunity conferred by the filtered muscle juice a virus of the strength of Arloing's second vaccine heated to 85 or 90 degrees Centigrade for 6½ hours was used. Three protected guinea-pigs showed no ill effects after injection of five centigrammes of this material. It did not follow that they would have developed black-quarter, supposing they had not been previously injected with muscle juice. One of the three was inoculated twenty days afterwards with a solution containing 2 mm. of acetic acid and one centigram of dried muscle juice heated to 85 degrees for 6½ hours. This guinea-pig died of blackleg in 15 hours. Six others, which I thought might prove refractory, succumbed to black-quarter in times varying

from 15 to 50 hours after the injection of Arloing's second vaccine mixed with acetic acid. The four protected (?) guinea-pigs resisted Arloing's second vaccine when it was not mixed with acetic acid. I have not tested the effect of mixing the second vaccine with other microbes, but their predisposing action is admitted, and their accidental presence accounts for a good number of the cases in which black-quarter has immediately followed upon preventive inoculation. Moreover, it is interesting to note that Hess, in his communication on "Preventive Inoculation" to the Sixth International Veterinary Congress, 1895, stated that the number of deaths following the first inoculation (first vaccine) amounted to about two-thirds of the total number which died of inoculated black-quarter. The first vaccine was prepared by heating the muscle juice to a dry temperature of 100-104 degrees Centigrade for 6½ hours. It ought, therefore, to be still more attenuated than the second, which was only treated to 85 degrees Centigrade. I have already said, however, that I do not think that the actual virulence of the black-quarter spore itself is materially altered. The smaller death-rate —one-third of the total—from the inoculation of the second vaccine is probably due to the animals having acquired a small degree of immunity after the first operation. The immunity, however, derived from the two operations was by no means absolute, because a certain number of vaccinated animals died of naturally contracted black-quarter. Hess reported the death-rate at 5 per cent. among inoculated animals, many naturally contracting black-quarter. The statistics of results of inoculation presented to the Fourth International Congress would be found in an article of my own, published in *The Journal of Comparative Pathology and Therapeutics*, September 1897, "The Preventive Measures directed against Black-Quarter," &c., and in one by Professor M'Fadyean in the same journal. In discussing the question of infection one must not omit the possibility of an animal organism being more susceptible at one time than at another. It is quite conceivable that an animal's leucocytes are not always in good working order, and I do not think it is always the most thriving beast that is struck. It has been seen that there were several substances which exert a predisposing action when introduced from without. Much less is known about the predisposing substance originating inside the body, but their existence is quite conceivable. Seeing that the microbes, or rather the spores, are present in the soil, I have sought to discover where the virulence of infected soils is influenced by the same conditions as the virus taken from the tissues. Two years ago I poured about 300 cc. of washings from a black-quarter muscle of an ox over the dried soil in three flower-pots. Owing to other affairs I have not been able until a few weeks ago to test the virulence of the earth. The pots have since remained in a greenhouse exposed to sunlight. Here, then, are the experiments I performed to test this virulence of the soil. All these operations were done with the necessary antiseptic precautions. They were performed on the 15th February. Two guinea-pigs were used. No. 1 received 3 cc. of washings from the soil which was taken 4 inches from the surface of the pot. The water used for washing the soil had been sterilised. The material was injected into the subcutaneous tissue of the left thigh, and the animal died on the 16th, or after about twenty hours. There was no discoloration of the muscles. An œdematous fluid was present under the skin, but it was clear; black-quarter œdema was red. Cover-glass preparations showed many cocci forms, but no bacilli. The experiment was a failure, but one must expect

failures because the soil was very impure. No. 2 guinea-pig received 3 cc. of the same material to which acetic acid had been added. The animal died on the 16th. The muscle from the thigh up under the belly was much discoloured. It was a red œdema. I made a cover-glass preparation, and it contained bacilli which were indistinguishable from those of black-quarter organisms. They were not in pure cultures. That animal, we may conclude, died from black-quarter. Next day I took the other two guinea-pigs, which I will call A and B. A received 2 cc. of washings from the same soil after it had been heated to 85 Centigrade for one and a quarter hours. The result was negative; the guinea-pig remained perfectly healthy. Guinea-pig B received the same material as A, but with a few drops of acetic acid added. The animal died 20 hours afterwards. The muscle was discoloured, and the bacilli like those of black-quarter were found in the muscle juice. We may conclude that that pig died of black-quarter. On February 26th I performed another series of experiments on three guinea-pigs, which I will label 1A, 2A, and 3A. 1A received into the the shoulder 1½ cc. of the washings from the same treated soil. The animal lived in perfect health apparently. 2A received into the shoulder 1½ cc. of the same material, but with acetic acid added. This animal died on the 28th, after thirty hours, with lesions of black-quarter. The bacteria found were indistinguishable from those of black-quarter. The animal had been dead some time before the post-mortem was made, and the lesions were invaded by other microbes in large numbers. It is difficult to prevent this in black-quarter in guinea-pigs, as the skin softens soon and allows the germs to pass. 3A received into the shoulder 1½ cc. of the same washings and a few drops of a broth which had been sown with ordinary fresh milk and incubated over-night. My object in taking milk was to see if organisms taken haphazard would not answer. This animal died thirty hours afterwards. There was a circumscribed swelling at the seat of inoculation. There was a putrefactive odour and small patches of green discoloration, and extending all along the subcutaneous tissue of the belly was a sanguineous œdema. The abdominal muscles and those of the shoulder were a dark red, as in cases of black-quarter. Cover-glass preparations made from the muscles showed bacilli like those of black-quarter. Well, to go back to 1A, I experimented seven days after. Into the shoulder I injected 1½ cc. of washings from the same treated soil, plus acetic acid. The animal died eighty hours afterwards. There was a slight sanguineous œdema and discoloration of muscles along the abdomen and shoulder. The lesions were not marked, but bacilli, like those of black-quarter, were present in preparations made from the muscle. Eighty hours was a long interval between the inoculation and the death. Possibly the first inoculation had given some degree of immunity. I merely say it was possible. I do not assert such a thing on the strength of one experiment. I should continue the experiments to see if antiseptics would render the soil innocuous. In a paper read before the Midland Society in 1897 I said I had often advised disinfection of the spot where the animal died. It was not until it lay down and died that the blood began to flow. Disinfection would probably kill the auxiliary microbes. Lime might be tried. I have never tried it, but should like to hear, if any one has, what the results have been. I am going to disinfect the infected soil with carbolic acid, and to take the washings and inject them into the guinea-pig, and see if that produces black-quarter or no. If not, we may take it that carbolic has done

something. If the acid has not killed the spore it might have at least got rid of the other microbes. These experiments which I have described are not very numerous I must admit, but with all necessary reserve on that account I am inclined to believe (1) that the black-quarter microbe is a saprophyte which of itself is feebly pathogenic; (2) that under natural conditions of infection it only becomes a parasite with the assistance of other microbes, or possibly through other agencies developed inside the animal; and (3) that most of the accidents which follow preventive inoculation are due to an impure virus being used, or the neglect of adequate antiseptic precautions in the performance of the operations.

Clinical Paper.

ECZEMA CHRONICA FACIALIS ET LABIALIS.

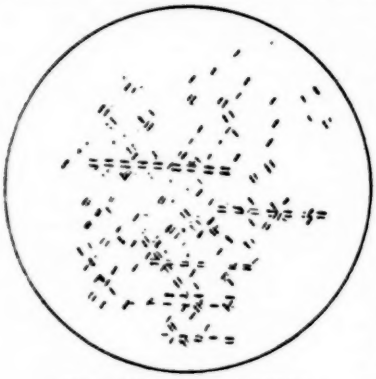
BY J. M'PHAIL, ESQ., M.R.C.V.S., INSPECTOR OF MARKETS, EDINBURGH.

THE following case may be of interest to your readers. I came across it while examining the animals at the fat stock sales in Edinburgh. In one of the sheep pens I noticed an old black-faced ewe in a most distressing condition; she was very much emaciated, with pendulous abdomen, and was breathing with extreme difficulty. Further examination showed that the sheep was suffering from an extensive disease of the lips and face; blood and pus were coming down the nostrils, and coagulating at the orifices: this, along with the very much swollen condition of the face, was producing the distressed breathing. As an animal in this condition was unfit for food, it was seized under the Public Health (Scotland) Act and destroyed.

When the animal was slaughtered, the carcase was thin and dropsical, due undoubtedly to the difficulty the animal had in eating and the malnutrition consequent thereupon. The skin of the face was swollen and indurated and studded over with papules, some of which contained pus, others a clear fluid; while on the lips, interior of the mouth, and some parts of the skin of the face, these papules had evidently burst and left small superficial ulcers of irregular shape. The disease here was evidently of long standing, as the bones of the face were softened and rarified; one could easily cut a piece out of them with a knife. The teeth were also quite loose.

On cutting up the carcase it was found that there were two large abscesses at the angle of the jaw, and that the glands of the thoracic viscera were swollen, softened, and infiltrated with serosity. The presence of the abscesses and the condition of the glands of the chest caused me to think that this was not a common case of eczema caused by the "*Odium albicans*" which gets the blame of causing this condition on the face of sheep. I therefore carefully sterilised a knife, and cutting down into the softened bone, first inoculated a tube of nutrient gelatine, and then emptied it into a Petri's dish. In 48 hours there were a large number of colonies visible, milky white in colour, with regular edges. A cover-glass preparation from them I found rather difficult to stain. I tried 2 per cent. aqueous solution of Thionin blue, also Löffler's blue, but without good results.

The most satisfactory stains were alcoholic solution of gentian violet and carbo-fuchsin. When stained with either of these, small rod-shaped bodies were seen (mostly in pairs), with slightly rounded ends. The accompanying sketch shows their appearance when stained with gentian violet and examined under a $\frac{1}{12}$ oil immersion lens with a No. 4 ocular. They did not show any spores in any of the specimens I examined.



Stain—Alcoholic gentian violet.
No. 4 ocular and $\frac{1}{12}$ oil immersion.

I also made a culture from the abscesses at the angle of the jaw and from the ulcers on the lips. That from the jaw produced a staphylococcus which when transferred to potato grew similarly to the *S. aureus*. A microscopic examination showed that small rod bodies were also present along with the cocci. The scraping from the lips produced simply cocci bodies, which I did not further investigate.

Further experiments of course ceased, because one can't go on inoculating sheep to see if the disease can be reproduced, but still the case is, I think, of interest, inasmuch as, if a similar condition be encountered, it will be interesting to endeavour to find the microbe and compare it with the one in this case. I must say that since I have come across this one I have my doubts as to the *Odium albicans* being the cause of this condition in sheep, although it probably is the cause in horses, especially if they are eating Alsike clover.

Notes.

VERIFICATION OF DOUBTFUL CASES OF ANTHRAX.

The following Memorandum has been issued from the Factory Department of the Home Office, and although chiefly directed to the medical man, it is not without interest for the veterinarian.

"Arrangements have been made, in order to give facilities to medical practitioners, subject to the conditions specified below, for bacteriological examination in cases of suspected anthrax in which the infection is believed to have been contracted in a factory or workshop. The conditions are that the approval of the Medical Inspector of Factories (Dr. T. M. Legge, Home Office) be obtained in each instance, that upon receipt of such approval the material be taken and packed by the practitioner, without delay, in accordance with the directions appended, and sent forthwith to the bacteriologist appointed for the purpose by the Home Office, with a full statement of particulars in the prescribed form, of which a copy is appended. Where the the practitioner considers this course advisable he should report the case (under s. 29, 1895, in the ordinary way) as probably due to

anthrax contracted in a factory or a workshop, adding that the diagnosis is doubtful and is not being followed up by bacteriological examination, and that he is prepared to forward samples.

"Internal anthrax can hardly be diagnosed with certainty during life. This can be best done after death, by means of bacteriological examination.

"The symptoms of external anthrax (malignant pustule) are, during life, in most instances, sufficiently characteristic to admit of hardly any doubt. In some cases, however, they may differ but slightly from those produced by erysipelas or other inflammatory processes.

"For complete proof it is desirable that in every case of anthrax the presence of the bacillus should be demonstrated under the microscope. In many cases, and especially in those reported from general hospitals, this is already done, but there remain a certain number of doubtful cases among persons employed in factories and workshops, in which the medical practitioner may desire to obtain such confirmatory evidence, or in which his own examination has failed to satisfy him that the case is one of anthrax. It is in such instances that the Home Office desires to afford facilities for verification of the diagnosis.

"The following directions should be observed in the collection and despatch of material:—

"*External Anthrax.*—If excised the pustule is to be placed untreated, wrapped in a little cotton wool or lint that has been boiled, in a small glass bottle.

"If the pustule is not excised, lymph from a vesicle, or from the exudation, should be collected in sterile capillary vaccine tubes, the ends of which should then be sealed in a flame in the usual way.

"Cover-glass preparations, made by smearing with a small quantity of exudation, and then dried without staining, may also be sent with advantage.

"*Internal or Fatal External Anthrax.*—Portions of the lungs and spleen, and the pustule itself (if the case be one of external anthrax), and samples of pleuritic, pericardial or peritoneal fluid, collected in tubes, as already described, should be sent.

"The regulations of the General Post Office require that all deleterious liquids or substances, sent for medical examination or analysis, shall be forwarded by Registered Letter Post under the following conditions:—

> "Any such liquid or substance must be enclosed in a receptacle hermetically sealed, which receptacle must itself be placed in a strong wooden, leather, or metal case in such a way that it cannot shift about, and with a sufficient quantity of some absorbent material (such as saw-dust or cotton wool) so packed about the receptacle as absolutely to prevent any possible leakage from the package in the event of damage to the receptacle.
>
> "The packet so made up must be marked 'Fragile, with care,' and tendered at a Post Office for transmission by Registered Letter Post. It must on no account be dropped into a letter box, or sent by Parcel Post. These regulations will be rigidly enforced. Any postal packet of the kind found in the parcel post, or any postal packet of the kind, whether registered or not, found in the letter post not packed as directed will be at once stopped and disposed of as the Postmaster-General shall direct.
>
> "Any person who sends by post a liquid or substance for

medical examination or analysis otherwise than as provided by these regulations is liable to prosecution, even if he be a patient sending something to his medical adviser for his opinion, or a medical practitioner sending something to a laboratory or elsewhere.

"The approval of the Medical Inspector, whether in the form of letter or telegram, should be forwarded to the bacteriologist named therein (as his authority for proceeding with the examination) with the samples and the particulars of the case."

CHEMICAL FERTILISATION.

The era when the male will be nothing and the female everything is close upon us. Such at least is the impression obtained by reading an article entitled "Science nears the Secret of Life," which appears in the current number of the *Humanitarian*. It appears that some time ago Professor Norman discovered that unfertilised eggs of certain marine animals began to develop if salts of magnesium were added to the water in which they were kept. Profiting by this hint, Professor Jacques Loeb, of the University of Chicago, obtained eggs from a female sea-urchin which he was certain had not been fertilised. These eggs were placed in water to which certain salts had been added. Calcium, sodium, potassium, and magnesium salts were employed. It was found that in the calcium solution no development occurred in the eggs. "When potassium was used they developed slightly. In sodium they slowly evolved into blastulæ, but in magnesium solutions these later forms were produced in two hours, and within twenty-four hours these blastulæ, having been placed again in normal sea water, developed into active and vigorous plutei. The experiments were a complete success, and magnesium chloride the successful medium for development. Not content with his own assurance that the eggs were entirely unfertilised, Professor Loeb took from each group used a portion which he immersed in sea water to develop in the natural manner, and in each case they remained unchanged, showing no growth."

Professor Loeb argues that if the egg of a sea-urchin can develop independent of the male factor, it is reasonable to believe that so also can the egg of a mammal.

Breeders and others had better keep their eye on these experiments, for it will undoubtedly be much cheaper to purchase magnesium chloride from the chemist than to maintain an expensive stud of breeding males.

HONOUR FOR A LADY SCIENTIST.

On Saturday, April 14th, the University of Edinburgh stepped out of the ordinary groove and honoured both itself and all lady scientists by conferring the honorary degree of LL.D. upon Miss Eleanor A. Ormerod. The name of Miss Ormerod, we may claim, is a household word among those interested in agriculture in general and entomology in particular. As consulting entomologist to the Royal Agricultural Society of England Miss Ormerod has rendered invaluable service to science. In no small measure has she contributed to veterinary knowledge as well as to more general agricultural subjects. We are sure that all our professional brethren will rejoice with us that the University of Edinburgh has singled

out this eminent woman of our time for an honour which is enhanced by the circumstance that this is the first honorary degree conferred by our northern university upon a member of the fair sex.

HORSES AT THE FRONT.

The terrible condition of the horses which have gone through the hardships of actual warfare is graphically told by the *Daily Telegraph* correspondent in a letter written from Stellenbosch under date March 27th, in which he describes the inspection of horses which had been returned from the front. "After a night's hospitality at the pleasant hands of my Colonial-born host and his family, work began in the grey of the morning by the driving-in of some 200 of the animals to be inspected. I will attempt no vividly drawn picture. But had you seen those bags of bones looming through the twilight on their way to the morning feed, it would have startled you no less than it did me, and would have brought before you more than any sight not actually in the fighting line could have done a picture of the horrors of war. I take it for granted that you have sympathies for the horse as well as for man. The less decrepit among this awful band of skeletons had been back from the front about a month, the worst cases about a fortnight—representing, with others I was yet to see, in great part the remnant of General French's good cavalry chargers with which he forced his way to Kimberley. How those ragged frameworks still supported life, even on the improved diet of their present quarters, was in itself a marvel. Yet most of them moved forward, some even blithefully, to their food—squeezing their gaunt projecting hips into the crush along the feeding-trough. Some few still bore signs of wounds scarce healed. Their detrainment at Picquetberg Road must have been a terrible experience. Several were dead in the train. Eight died at the station; a few more on the way to the farm; while on most days afterwards one or more would succumb from sheer weakness. It was the same at the next farm and at the third. I care not to call up the remembrance of those angular, almost transparent, frames and tottering limbs. Of the whole 600 only four could be found against whose hoof-numbers one could write 'fit for service.' Of the others, not half are likely ever to do service again for Her Majesty. The remainder, it is pleasing to know, are revelling amid streams of cool water in wide if not very luxuriant pasture, and twice a day come to the troughs for what the Americans term 'a good square meal.' For my humble part, I should venture to consider the meal both better and squarer if its component parts were more suited to digestive organs already enfeebled by starvation. Wheat, 8 lbs., and straw chaff unlimited (as per contract), are hardly calculated to coax back flesh and stamina exhausted by prolonged fatigue and hunger. But this point, I have reason to believe, has been duly represented in the proper quarter, and is likely to be remedied. For—it is satisfactory to add—out of about 150 similar cases arriving at Stellenbosch Camp a month or five weeks ago, and there fed freely upon linseed, bran, and oats, more than forty have already returned to the front in tolerable working condition."

CAMBRIDGE AND TUBERCULOSIS.

The science schools of Cambridge have of late set the best of examples. They have not been content with mere research work, but have sought to

put the knowledge gathered in the laboratories to practical use. The lengthy series of investigations upon the question of tuberculosis and milk carried out by the late Professor Kanthack and Dr. Sladen conclusively proved, with those of Professor Bang of Copenhagen, that we have in our midst an ever-threatening foe with which we have not yet begun to grapple, though it costs us annually as many lives as the Boer war. That milk is a potent factor in the spread of disease has now become a matter of common knowledge. It is rather characteristic of the slow ways of our nation, of our unwillingness to adopt new ideas, that as yet little or nothing has been done to counteract this evil. Perhaps we are paralysed by the magnitude of the infection in our herds, and by the question of compensation. But if we wait for the powers that be to take the initiative we shall get to know, like the poet Spenser, what it is "to lose good dayes that myght be better spent." Cambridge, recognising the fact that in England things are best done by private enterprise, has wisely taken the matter into her own hands and established a Tuberculosis Committee, which hopes to attain its aim by the voluntary co-operation of the farmers and dairymen, combined with subscriptions from the university and town. The Town-Council agree to pay the expenses of a veterinary surgeon, and the County Council, it is hoped, will also aid the undertaking. The methods adopted are particularly those already obtaining in Denmark, viz., complete sanitation of the cattle, authorised tuberculin testing, segregation of the healthy, separation of calves born of reacting cows, interdiction of service between the unhealthy and healthy, and so forth. It is not merely sterilisation of the milk supply that is aimed at, but complete extirpation of the disease. It is to be hoped that this experiment, initiated solely by the scientific workers in the University, and practically organised by them, may be brought to a successful issue. We understand that Oxford has in contemplation the adoption of a similar scheme.—*The Field.*

THE PREVENTION OF TUBERCULOSIS.

At a recent meeting (March 13th) of the National Association for the Prevention of Consumption and other forms of Tuberculosis, a report was presented in which is set forth the work done since the meeting held on May 4th, 1899. It appears that, despite correspondence urging the necessity of applying pressure in the direction of inducing the Government to make more stringent legislation in respect of the inspection of meat and milk, the Council of the Association has deemed it more advisable to direct its efforts chiefly towards the enlightenment of public opinion. Such efforts are no doubt praiseworthy in the extreme, but it seems a pity that something more radical could not have been at least attempted. It is of course necessary to bear in mind the difficulties which must have been met with by a newly organised association; and therefore our criticism should not be too harsh. We therefore only remark that we expect greater and better things from the Association in future. The presence of more members of the veterinary profession in the Association we feel would be of the utmost value; we therefore beg to urge our professional brethren to become members, and to promote the interests of the Association by all the means in their power.

Extracts.

PERIPROCTAL ABSCESS IN THE COW.

On the 16th December last I was called in to see a cow about eight years old. The owner informed me that three weeks before she had calved normally and easily, but showed purulent discharge from the vagina, ate badly, ruminated incompletely, and for some days had been evincing pain on defæcation and micturition.

The cow was fairly well nourished. The coat was rough and dull. At the inferior commissure of the pudenda was clinging some white-yellow thick pus. On examination per vaginam there was found a small quantity of similar pus at the *os uteri*. The latter was only big enough to admit one finger. The bladder had an empty feeling. The cow evinced no pain on palpation of the latter. In the posterior part of the rectum there were pulpy yellowish-brown fæces. About 40 cm. in front of the anus the rectum was narrowed down into a funnel-shaped tube. Through the narrowest part only three fingers could be introduced. On the left side of this narrow portion could be felt between rectum and the ilium a tumour, non-fluctuating, hard, and as big as a man's head, on palpating which the animal groaned. I introduced through the *os uteri* the thin tube of an irrigator and sprayed in several litres of a weak creolin emulsion. As to the tumour, I told the owner that it would probably some time or other soften, and might then perhaps be voided from the rectum.

On 23d December I again examined the cow, and found on exploring the rectum that the tumour was moderately fluctuating. I then made use of a small narrow lancet by which to make an incision on the fluctuating spot. Just as I was about to apply the knife to the wall of the rectum on the left side and in a somewhat ventral position, it suddenly burst and there was voided per anum quite a litre of gray-yellow, stinking filthy pus. The longtitudinal tear in the rectum was about 6 cm. long. Through it the fingers could reach a big smooth-walled cavity. This was thoroughly washed out with creolin emulsion, the irrigation being repeated on 24th and 28th December. Complete cure resulted.

(*Berl. Thierärzt. Wochensch.*)

SOME AFFECTIONS OF THE HEART IN RACEHORSES.

In a communication made to the Central Society of Veterinary Medicine, M. Cagny has brought out some very interesting points relative to an affection contracted by racehorses, which he calls "strained heart." Our *confrère* has devoted himself first of all to proving how painful and sickening to colts of eighteen months the work of training is, and how often this exercise, in consequence of the exigencies of the proprietors, is unmethodical and little understood. He also reminds us how irregular is the running of certain horses which are spoken of in racing circles as being unable to make a struggle in the final effort of a race. It is frequently said about these horses that they have not "heart enough." Is this the effect of a lack of will-power in the animal, or is it an organic affection of the heart? M. Cagny is tempted to a belief in the second hypothesis, basing this on the well-known fact that certain horses can,

without injury to themselves, make this final spurt in one or more races, and then one day, after some great effort, are no longer able to hold their own against horses of very inferior calibre. The author thinks that the horses develop what he calls "strained heart," an injury comparable to that which young men are liable to who go in for exaggerated bodily exercises, such as bicycling, walking, &c.

M. Cagny reports having treated with success several horses afflicted with this trouble. It is necessary, however, to add that he does not claim to be able to state the symptoms of this affection in any precise manner. He has simply ascertained functional disorders of the heart in his subjects, troubles which have generally disappeared after appropriate treatment, such as repose, followed by moderate exercise, and the adoption of other tactics whilst negotiating a race.

Having noted the communication made by M. Cagny, M. Trasbot presents some reflections on the injury to the heart of a horse following on great and repeated efforts. He thinks the affection variously called "tired heart" or "strained heart" is most frequent in those horses from which violent efforts are demanded. He explains this lesion of the heart by the expenditure of energy necessary in struggling against the pulmonary obstruction so frequent in this kind of exercise. M. Trasbot has had opportunities of examining a number of animals afflicted with this organic lesion of the heart. The symptoms at the beginning of the complaint are vague, the most characteristic being a diminution of muscular power as well as a diminution of the power to stand work. The horses quickly become winded, and turn lazy. But there is one symptom, says M. Trasbot, which constitutes a certain sign of "tired heart;" this is a reduplication of the first heart-sound. This phenomena is frequently observed in simple pneumonia, the heart having to fight against an obstruction of the lungs. In the latter ailment, the cause of the obstruction being temporary, the heart affection disappears with the lesion causing it.

This reduplication of the heart-sound appears to be due to the fact that the systole of the right heart, which specially struggles against the pulmonary obstruction, takes place a little after the systole of the left heart. This fatigue is often followed by a dilatation of the right ventricle, with or without hypertrophy of the wall; later this injury is accompanied by valvular lesions. M. Trasbot is convinced of the frequency of this disease in racehorses.

(*Bulletin de la Soc. Centr. de Méd. Vét.*).

Reviews.

THE COMMON SENSE OF PHEASANT REARING. By SPRATT'S PATENT, LIMITED, London.

Those of our readers who are interested in the breeding and rearing of pheasants and other game will find much which will repay perusal in a small publication having the above title, a new edition of which has just been issued. The diseases of game are treated of in a short chapter, and hints on the management of deer are given by a practical park-keeper. In addition such subjects as pheasant driving and the practical management of

a sporting kennel are briefly but lucidly discoursed upon. The pamphlet should appeal strongly to all sportsmen and those who have to do with game. Messrs. Spratt's Patent, Limited, will be pleased to post a copy to any of our readers who will drop them a post-card.

BIBLIOGRAPHIA MEDICA (*Index Medicus*). Paris: Institut de Bibliographie.

In these days in which the current literature on any scientific subject can only be described as enormous, it is absolutely necessary that the workers in various departments should have some means by which they may gain a knowledge of what is being done by their co-workers, and that with as little expenditure of time as possible. From 1879 to 1899 the *Index Medicus* published in the United States fulfilled such a purpose. This publication, however, ceased to appear last year. The *Bibliographia Medica* has now taken its place. This is in reality a monthly list of the papers on medical and allied subjects which have appeared in the periodicals of the world during the preceding month. In order to facilitate reference the papers are classified under various headings, and we notice that there is a section devoted to Veterinary Medicine. The *Bibliographia*, if we are to judge from the numbers which have already appeared, promises well, and there can be little doubt of its success.

TRANSACTIONS OF THE HIGHLAND AND AGRICULTURAL SOCIETY OF SCOTLAND. Vol. XII. 1900.

The recently published *Transactions of the Highland and Agricultural Society* contain three papers which will appeal to veterinary surgeons.

The first of these is on "The Dangers of Water-Drinking," by Professor W. Owen Williams. The opening sentence gives the key to the whole paper. "The goodness of water for human consumption has ever been carefully attended to, but to that for the lower animals little heed has been paid. In fact, I may say with considerable truth that anything *fluid* has been considered as being equivalent to water for them." This theme is elaborated, and suggestions made for the removal of the existing undesirable condition of affairs. "In conclusion, I would advise that all drinking water, and water used for mixing with food-stuffs, be obtained either from a hill loch, a quickly running stream, a deep well, or from below a deep sand and gravel bed; that it be run through a sand filter before being stored in a slate or stone-built cistern, thus making certain that a considerable number of dangers to the health of the consumers be obviated."

Professor Wallace contributes a paper on "Scab in Sheep: Suggestions for its Eradication." The following is a brief summary of the regulations which the writer believes will be required in the final supreme effort to eradicate scab after an Order for compulsory dipping has done its preliminary work:—

"(1.) A dipping season to be fixed between the 1st of June and the middle of November.

"(2.) After local enquiry the county to be subdivided into convenient areas, the conditions of which make it desirable that the flocks in each should be systematically dipped within a period of fifteen days, which might be regarded as sufficient control in all districts believed to be free from scab.

"(3.) Where scab exists within an area a second dipping to be made compulsory between the fifth and the fourteenth day after the first dipping of any given flock of sheep.

"(4.) That inspectors be appointed by the Board of Agriculture who have a thorough practical knowledge of the management of sheep. Each inspector to control the working of one or more areas under general regulations formulated by the Board of Agriculture, but with sufficient elasticity to enable them to be made applicable to local conditions.

"(5.) No sheep to be removed from one area to another during the dipping season of five and a half months without being first dipped immediately before removal—once in the case of clean areas, and twice in the case of areas in which scab exists.

"(6.) Flocks which have travelled or have been to market to be dipped not later than the seventh day after being imported into an area and before coming into contact with other sheep.

"(7.) All railway trucks and pens in public markets and other places in which sheep are placed to be thoroughly disinfected—first, say with carbolised water, or a solution of corrosive sublimate, and afterwards with fresh lime whitewash.

"(8.) Due notice to the inspector to be given in the case of infected farms of the dip material to be used and of the date and place of dipping of the sheep to be removed, so that if he considers it necessary he may send a sub-inspector to supervise.

"(9.) Effective dip material to be used, and where close inspection and control are necessary, the first dip to be coloured with Venetian red or Spanish brown, and the second dip with ochre.

"(10.) The County Council to be authorised and directed to provide dipping-tanks, the total cost to be met by those who use them."

The third paper is by Professor Dewar, who writes at some length on "The Horse's Foot, and how to Shoe it."

AMONG HORSES IN SOUTH AFRICA. By M. H. HAYES, F.R.C.V.S. 5s. London: R. A. Everett & Co.

We expected it. It was not to be supposed for a moment that, when all and sundry are writing on South Africa and South African affairs, the veterinary profession would remain silent. Therefore we have been looking forward to the appearance of a book of some sort written by a veterinarian. And here we have it. Captain Hayes, than whom we have no more prolific writer, has given us a publication with a slightly misleading title. Misleading because the "Among Horses" element is not the dominating factor. "My South African Experiences and Impressions" would have been a far fitter heading. Probably that which will interest the reader most is the impression Captain Hayes received of the political and economic state of the Boers. Concerning the Boer farmer the author gives us much interesting information. "The custom in South Africa is to have immense farms, anything under 50,000 acres being considered a small one. A Boer does not feel comfortable if he can see from his own house the smoke from any other man's chimney. Fortunately, the ambition to be a large landed proprietor in that part of the world can be easily gratified, for land can be bought right out for half-a-crown an acre and upwards. Owing to want of water much of the land is unproductive, and the high

price of labour restricts the amount of tillage within very narrow limits. Despite high protective duties, the most of the corn, and even of the flour, is imported. So backward is the practice of agriculture, that market gardening is almost unknown except in Natal, where it is assiduously and profitably carried on by natives of India. The most of the vegetables eaten by white men are imported in tins. So scarce are fresh vegetables in up-country towns, that, as a rule, those which come into the market are auctioned off singly. It was nothing uncommon to see a cauliflower, which in England would cost twopence, knocked down to the highest bidder at three shillings. . . . The farmers have few wants. A suit of clothes lasts them for years. They don't buy books, back horses, smoke any tobacco which they don't grow, drink any liquor which they don't make, or eat any food which their farms do not produce. . . . I am afraid that some of them are just a little lazy, for I have known several instances of these gentlemen buying harness horses when they had hundreds of animals running wild on their land, but they did not care to take the trouble of catching those they require and breaking them in."

The present-day Cape horse is apparently but a sorry specimen of the equine race. "The Cape horse of forty or fifty years ago was a strong serviceable animal that was well up to remount form, and was prized in cavalry regiments in India. At present he is as extinct as the quagga. His place is now taken by a weedy slave, who, though sound and hard-working, is singularly deficient in spirit and strength." "Throughout South Africa there is an immense number of horses raised in a semi-wild state on the various farms. Very few of these animals would be, from an English, Australian, or Indian point of view, worth breaking, for the privations they have to undergo from want of grass make them weak and listless. . . . Such horses, even if they could be obtained for nothing, would not be worth exporting to India or to any other country." The breaking in of these horses cannot be said to be a very elaborate process. "It consists of tying the head of the neophyte close up to that of a steady horse by means of a cord connecting the respective headstalls worn by these animals. After they have both been saddled and bridled, the 'schoolmaster' is first mounted, and then another man gets on the young one, who is powerless to buck, rear, or run away, on account of his head being fixed. . . . As he settles down quietly to work, the connecting cord may be gradually loosened out, until at last it can be taken off altogether." There can be no doubt that the lack of enthusiasm in horse-breaking in South Africa is to a considerable extent due to the prevalence of the terribly fatal "horse sickness," a disease which annually carries off somewhere about 20,000 horses. "The disease, which is almost always fatal, is characterised by the escape of immense quantities of watery fluid from the blood-vessels into various tissues. When the lungs are the chief seat of the malady, the fluid accumulates in the bronchial tubes, and is discharged more or less as froth from the nostrils. At last this means of relief proves inefficient, and the animal becomes suffocated, or, to use Edington's expression, drowned in his own blood serum. The disease runs its fatal course usually in about four days. . . . . The elaborate and very skilful investigations made by Edington strongly support his conclusion that horse sickness is caused by a fungus that grows on herbage, and that this microbe attains its virulent form under certain conditions of moisture, such as we find in dew. Consequently, in infected districts it is almost certain death for horses to graze while dew is

on the ground. Hence the best preventive measure is to feed horses only on dry forage. If they have to stop out at night, they should, if possible, be kept in an enclosure (or *kraal*) in which there is no herbage. Edington tells us that this disease appears to occur every year in some parts of the colony, and that it assumes the form of an epidemic only at wide intervals of time, which is lucky, considering that in 1854-55 64,850 horses died of it, and 13,979 horses when I was there in 1892. February, March, and April are the worse months."

Most of us have either known personally, or have heard on good authority, of cases of strange friendship between animals. Captain Hayes gives us four new examples. On one occasion when he was giving a horse-breaking exhibition in Kimberley, one of the animals to be broken was followed into the ring by a goat, which tried to butt every one who attempted to separate him from his equine friend. "The goat appeared to take an intelligent interest in the proceedings, and as soon as I had finished with his companion, and had driven him out of the enclosure, the goat followed him, and the strange pair went off at full gallop to the distant pasturage from which they had been taken. It is worthy of remark that this friendship had been cemented while these animals were in a practically wild state. While at Cape Town I noticed the existence of a friendship between a horse and a cow, both of whom, by keeping constantly together, showed that they valued each other's society. . . . A racing pony mare which I owned, and which was barren, indulged in the kindly freak, when she had been turned out to grass in her old age, of adopting a motherless foal, and was never happy unless it was close to her side. Captain Beresford of the Royal Artillery once had in India a racing pony called Potboy, who would go nowhere unless he was accompanied by a particularly ugly and worthless pony mare, who accordingly spent her time walking about training grounds and attending race meetings at which her friend Potboy was engaged to run. If she did not go down with him to the starting-post, he would persistently refuse to go forward when the flag dropped."

Apparently the noble sport of horse-racing is in as bad a way in South Africa as the precarious game of horse-raising, and probably from much the same causes. "Racing in South Africa has a fine future, though a somewhat ignoble past. In order to flourish, it requires to be managed by men who are independent of it either for their livelihood or for their recreation, and whose probity and love of fairplay are above suspicion. It also needs a public that is wealthy, numerous, and fond of sport."

Captain Hayes has obviously written, mainly at least, for the general public; consequently, topics more general than those indicated above bulk very largely in his book. He discourses pleasantly and chattily on such widely different subjects as the De Beers Company and prize-fighting, Majuba and hints to public showmen. We gather that neither Cecil Rhodes nor Kruger is approved by our author. Seeing, however, that our quotations from the more "horsey" parts of the book have attained an almost inordinate length, we refrain from further extracts, and merely say that we do not always feel convinced by Captain Hayes' political arguments. We may be altogether wrong, but the impression we gain is that his conclusions have, sometimes at least, been arrived at somewhat prematurely.

The book will serve to pleasantly and profitably pass away an hour or two, and we can therefore commend it to our readers. Is it an inducement

to purchase when we say that the frontispiece consists of a portrait of Mr. Kruger and his spouse?

PRACTICAL HORSE-SHOEING. By G. Fleming, F.R.G.S., &c. 10th Edition. London: Baillière, Tindall, & Cox. Pp. 136. 8vo. 2s. 6d. nett.

The fact that this is the tenth edition, and that it treads so closely on the heels of its predecessor, is a sufficient guarantee of the popularity of this little book. Seeing that the present edition is merely a reprint, it is not necessary to devote a lengthy review to its consideration. Indeed it is only the young veterinarian who will not be perfectly acquainted with Dr. Fleming's book. For the benefit of the young man, therefore, let us merely say that the work at present before us first appeared in 1872, and sprang out of an essay which gained the first of three prizes offered by the Scottish Society for the Prevention of Cruelty to Animals for "the best and most practical essays on Horse-shoeing, in connection with the comfort and soundness of the Horse." It is not, therefore, so exhaustive in the treatment of the subject as the more recent works on horse-shoeing: but it contains much valuable information in a small compass. It probably appeals more to the farrier than to the veterinarian; nevertheless the latter may reap benefit from its perusal.

Correspondence.

ARMY VETERINARY DEPARTMENT.

To the Editors of the VETERINARY JOURNAL.

SIRS—Having read in the *Morning Post* the copy of a telegram from Headquarter Camp at Bloemfontein, referring to the manner in which the work of the Army Veterinary Department is conducted, I should, with many of my professional brethren, like to know how it is that the Army Veterinary Department, when put to the test, should show itself so thoroughly disorganised. On reference to the Army List, I notice there are now thirty Veterinary Majors, but only five are shown as serving in South Africa. How is this? Surely their experience would be of some value during this campaign, or has the Department always failed on previous occasions when the officers have been employed, for it is worthy of notice how many of these have crossed swords before their names, indicating that they have been employed on some occasion in the defence of their country. Would not the influence of these senior officers have been advantageous when it was found necessary to employ so many civilian Veterinary Surgeons? Again, why is this sudden dearth of candidates for the Army Veterinary Department? During the last two years it has been impossible in spite of the repeated efforts of the head of the Department, to persuade students to join. And yet, under the two previous Directors General, statistics show that the supply considerably exceeded the demand. Of course the introduction of the four years' course is bound to affect the number of students, but surely some of them would join the Army Veterinary Department, unless there is something radically wrong with the management and treatment of the members of the Department.

PRACTITIONER.

CONTAGIOUS DISEASES ANIMALS ACTS, 1864 AND 1899.

SUMMARY OF RETURNS OF OUTBREAKS for the Four Weeks ending 14th April 1900.

ENGLAND AND WALES.

Foot and Mouth Disease.	Swine Fever.	Glanders (including Farcy).	Rabies.	Anthrax.	Sheep-Scab (for month of March).
None.	Outbreaks 138 Slaughtered 1307	Outbreaks 78 Attacked 122	None.	Outbreaks 36 Attacked 69	Outbreaks 162

SCOTLAND.

None.	Outbreaks 3 Slaughtered 358	Outbreaks 0 Attacked 7	None.	Outbreaks 15 Attacked 22	Outbreaks 15

IRELAND.

None.	Outbreaks 30 Slaughtered 423	Outbreaks 1 Attacked 2	None.	Outbreaks 3 Attacked 9	Outbreaks 42

Communications, Books, Journals, &c., Received.

Veterinarian; Record; Bulletin of The Johns Hopkins Hospital; The Journal of Comparative Medicine and Veterinary Archives; The Scottish Medical and Surgical Journal; American Veterinary Review; The Journal of the Jamaica Agricultural Society; La Riforma Veterinaria (Naples); Recueil de Médecine Vétérinaire (Alfort); L'Echo Vétérinaire (Liège); Annales de Médecine Vétérinaire (Brussels); Berliner Thierärztliche Wochenschrift (Berlin); Deutsche Thierärtzliche Wohenschrift (Hannover and Karlsruhe); La Clinica Veterinaria (Milan); Agricultural Journal (Cape Town).

The Sanitary Journal; Le Mois Scientifique; Gazette Médicale de Paris; Bibliographia Medica; Journal of the Royal Agricultural Society of England; Farm, Field, and Fireside; The Common Sense of Pheasant Rearing; Canine and Feline Surgery; Practitioner; E. C. Russell, Grantham.

We regret that, owing to great pressure, we are unable to publish this month several important communications, &c. received from contributors.